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NOTE.

The authors of the several papers contained in this volume are themselves accountable for all the statements and reasonings which they have offered. In these particulars the Society must not be considered as in any way responsible.

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MEMOIRS.

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M = Memoirs. P = Proceedings.

Manchester Memoirs, Vol. lvii. (1912), *No.* 1.

I. The larger Coal Measure Amphibia.

By D. M. S. Watson, M.Sc.

(*Received and read October 15th, 1912.*)

The large Stegocephalia of the Coal Measures and Lower Carboniferous, although of very great interest, have never been satisfactorily described, and the present partial description of some of the material in the Newcastle Museum is to be regarded as a preliminary sketch, to be followed by a more fully illustrated memoir dealing with all the available material.

The material at Newcastle includes five more or less complete skulls of "*Loxomma Allmani*": and separate:—premaxillae, maxillae, lachrymal, supratemporal, frontal pre-vomer and palatine bones, one complete and seven incomplete rami of the lower jaw, including separate dentary and splenial elements, of the same form, which is probably not even generically identical with Huxley's type of the species.

The form described by Atthey as "*Anthracosaurus Russelli*" is represented by one complete skull and separate premaxillary, maxillary, nasal, pre-vomer, and palatine bones, and separate portions of skulls consisting of frontals, post-frontals, parietals, post-parietals, tabulares, supratemporals, and basisphenoid, parasphenoid, exoccipital, prootic and opisthotic, respectively. There are also two complete rami of the lower jaw and some other fragments. This type is very different from the original type of the species, and I intend to refer to it as *Pteroplax*.

In addition to the skulls there are very many vertebrae, ribs, clavicular elements, etc., which cannot generally be identified.

December 12th, 1912.

All the specimens are crushed quite flat but shew the sutures with admirable clearness. All the material is excellently prepared, almost all the bones being free from matrix.

Embleton and Atthey have described the best skull of "*Loxomma*" in a paper illustrated with very excellent figures. Their description of the upper surface of the skull is very accurate and detailed, except that they state that the alveolar border of the left maxilla is imperfect, when actually the whole bone has been disarticulated and is missing. Their description of the quadrate and palate are not satisfactory because they were misled by presumed resemblances to the crocodile. Reference to their figures will give all necessary information about the top of the skull. The palate is redescribed below.

The Basi-occipital is a small bone of conical shape, the base being formed by the single large condyle, which is concave, and exactly resembles the end of a vertebral centrum, which it no doubt is.

The greater part of the rest of the bone is covered with roughened surfaces for other bones, the back of the parasphenoid below and the exoccipitals above ; it is probable that it did not enter into the foramen magnum, its upper surface being completely covered by the two exoccipitals.

The Basisphenoid, Parasphenoid, and Ethmoid are fused together, and only their lower surface is well seen. The back of the basisphenoid is recessed for the anterior end of the basi-occipital, and what is presumably the posterior end of the parasphenoid projects backwards, covering the lower surface of the latter bone. The sides of the basisphenoid slope upwards, passing imperceptibly into the opisthotic and pro-otic. On each side of the bone in the region of the pituitary fossa is a powerful process, the processus basipterygoideus, which has a well-marked, smooth, articulating face on its anterolateral side: deeply impressed on the bone on the inner side of each of these processes is a groove which leads round from the side of the basis cranii to open in front into a foramen passing through the bone, which is undoubtedly the carotid foramen. In advance of this region the bone is laterally compressed and transformed into a deep plate articulating above with the roof of the skull and appearing on the palate for about half the length of the skull.

The Pterygoid is a very large bone forming the greater part of the palate. It articulates by a well-marked facet with the basisphenoid exactly as does that of *Sphenodon*. From this region it passes forward as a broad flat plate apparently touching the parasphenoid for some distance and certainly having a long articulation with its fellow in front. The posterior part of the pterygoid is bent round so as to reach the top of the skull. It unites with the squamosal to form a floor to the otic cavity and passes backwards with the squamosal to the quadrate.

The quadrate is a large bone, which only appears for a small area on the upper surface of the skull, where it is overlapped by the pterygoid and has a strong sutural attachment to the quadratojugal and squamosal by a much-thickened edge. On the under surface it has a larger exposure, passing upwards on the under surface of the roof of the skull until its upper end is received in a slit in the squamosal, which thus covers it both dorsally and ventrally. The posterior end of the bone is thickened and forms an articulating surface. The relations of the bones are shown in the drawing (*Plate*).

This is, I believe, the first complete account of the quadrate in a Carboniferous or Permian Stegocephalian.

Lying on the outer side of the pterygoid behind is the Transpalatine, a thin flat bone, whose outer border is thickened and articulates by a very loose suture with the maxilla. The anterior border of the bone is in contact

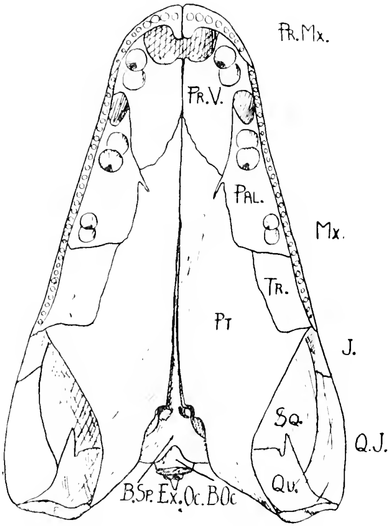

Fig. 1. Skull of "*Loxomma.*" Palatal aspect. × ¼.

B.OC., Basi-occipital. B.SP., Basisphenoid. EX.OC., Exoccipital. J., Jugal. MX., Maxilla. Q.J., Quadrato-jugal. QU., Quadrate. PAL., Palatine. PR.MX., Premaxilla. PR.V., Prevomer. PT., Pterygoid. SQ., Squamosal. TR., Transverse.

with the palatine for its whole breadth, there being no sub-orbital fossa.

The Palatine is very similar to the transverse in general character, but bears two large tusks near its outer border. Each of these teeth has associated with it a shallow pit from which a tooth has been shed, and in which a replacing tooth will be formed. In some cases both teeth are present at once, a condition which was undoubtedly only transitory; this curious type of tooth change is very characteristic of the Stegocephalia, and is unknown elsewhere except in the *Crossopterygian fish, where it occurs in a very typical form in the vomerine tusks of *Megalichthys*, and no doubt in many other genera, and in *Lepidosteus*. This occurrence seems to me a strong additional reason for regarding the Tetrapoda as derived from this group of fish.

The outer edge of the palatine has a long bearing with the maxilla, and the anterior end of the bone narrows and has a smooth edge forming the back of the posterior naris.

The pre-vomer is a large bone which meets its fellow in a long median suture, behind which it has a long articulation with the pterygoid, which extends backwards until it meets the palatine. These two bones are in contact until the posterior naris is reached, when they separate, the pre-vomer forming its anterior border. The lateral edge of the bone articulates with the premaxilla and maxilla, and in front it forms the back of the large anterior palatine vacuity. The pre-vomer carries one large tooth and the pit for its successor.

The Premaxilla has a very narrow palatal exposure, being solely represented by its tooth-bearing edge, which widens at the middle line into a short, blunt, backwardly directed process.

The Maxilla is entirely formed by a plate on the side

* Throughout this paper "Crossopterygian" is used as including only the three families *Holoptychiidæ*, *Rhizodontidæ* and *Osteolepidæ* of S. Woodward's sub-order *Rhipidistia*, and excluding *Tarrasius*, *Cœlacanthus* and *Polypterus*.

of the skull, whose lower tooth-bearing border alone appears on the palate, lying along the thickened edges of the pre-vomer, palatine, and transverse bones; it forms a portion of the outer side of the internal nasal opening. It is a curious fact that in *Loxomma* the palatine is fused with the lachrymal, although both bones are quite free from the easily detached maxilla.

Pteroplax is best represented by the skull which was described by Atthey as "*Anthracosaurus Russelli.*" His

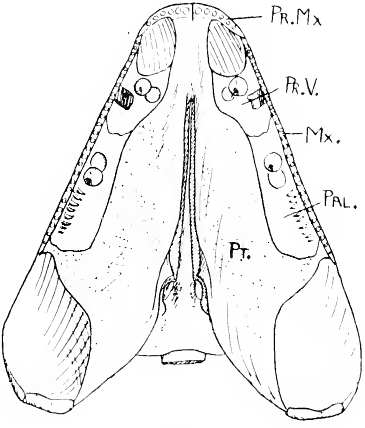

Fig. 2. Skull of *Pteroplax* sp. Palatal aspect. $\times \frac{1}{6}$.
Reference letters as on *Fig.* 1.

description of the upper surface is quite accurate, except that he treats part of the pterygoid as the quadrate, and taken in connection with his excellent figures gives a good idea of the general form of the skull.

I have never seen a Basi-occipital, but it must have been very similar to that of "*Loxomma*," except in appearing in the lower border of the foramen magnum.

The basis cranii is very similar in the two types, except that in *Pteroplax* the carotid grooves, after curving round the very large processi basipterygoidei, pass up the sides of the bone to enter the pituitary fossa.

The Exoccipital is a very small bone articulating below with the basi-occipital and above with the opisthotic. It does not appear to be pierced by any foramen. The opisthotic and pro-otic are fused. Their joint upper border has a distinct facet for the epiotic, which truncates the grooves for the semicircular canals, which are freely exposed on the inner surface of the bone ; the opisthotic also bears a short paroccipital process, which articulates with a very distinct facet on the under side of the tabulare. Between the pro-otic and the exoccipital is a triangular notch, which no doubt gave exit to VII.? IX., X. nerves. In front of the ear the inner surface of the pro-otic is smooth, forming part of the brain case. The bone ends anteriorly in a notched border, in front of which the other cranial nerves seem to have passed out. In this region the basisphenoid is excavated by the pituitary fossa, the sides of which are covered by a pair of processes which rise as they pass forward to fuse with the parasphenoid and ethmoid to form a thin vertical plate of bone, which runs forward in contact with the roof of the skull, nearly or quite to the premaxillæ. Reduction of this mass of bone would easily give rise to the Sphenethmoid, a bone which certainly occurs in some Stegocephalia (*Myriodon*).

The palate of *Pteroplax*, though very different in details from that of "*Loxomma*," has a fundamental resemblance that is very striking. The pterygoids in the two types are very similar, but in *Pteroplax* the articular facet for the basisphenoid is carried on a distinct process, and the bone appears to extend forwards to meet the pre-maxilla. The sutures in this part of the skull are not, however, very clear, and it is possible that the broad bar which reaches forward is really formed by the ethmoid or parasphenoid, the pterygoids being terminated by sutures. The greater part of the palatal surface of the pterygoid is covered with a shagreen of small sharp teeth.

There is no transverse bone, the Palatine extending back to the end of the maxilla. It is generally similar to that of "*Loxomma*," but instead of two large teeth has only one, the hinder being represented by eight or nine small teeth, arranged in a close set row parallel to the edge of the maxilla.

The Pre-vomer is a small bone bearing one very large tooth and a replacing tooth or its pit. Just behind and outside the tusk is a small notch which forms the anterior, internal, and posterior borders of the posterior naris. The appearance of two articulated skulls and an isolated bone seem to shew definitely that the Pre-vomers did not meet in the middle line.

The two types just described, which come from the Middle Coal Measures, agree in all their more striking features. The occurrence of a single basioccipital condyle, of very reptilian processi basipterygoidei, of very large pterygoids, which leave only a small inter-pterygoid vacuity divided by a narrow parasphenoid, separate them off very distinctly from all Permian and Triassic Stegocephalia and are certainly primitive features. The type skull of Anthracosaurus gives direct evidence that the same type of palate occurs in the Lower Carboniferous in the oldest known amphibian, and the type specimen of *Baphetes* gives less complete evidence of its occurrence in the Coal Measures of Nova Scotia. The lower jaw described by Moodie as **Erpetosuchus kansensis* is exactly

* The name *Erpetosuchus* is preoccupied by E. T. Newton, 1894, for a genus of Thecodonts from the Trias of Elgin

similar to that of *Pteroplax* and gives further evidence of the wide distribution of the type, and I have some evidence that in the Coal Measure Microsauria an analogous condition obtains. On the other hand no palate with large vacuities like that of *Eryops* or *Capitosaurus* has ever been found in Carboniferous rocks.

These characters of the palate, which I have shown above to be common to all the early Stegocephalia, are exactly the features which are depended upon to show the reptilian character of such a skull as *Seymouria*, and leave in my mind no doubt that the reptilia were separated off very early on in the history of the Stegocephalia, preserving features which were rapidly lost by the latter group, which had a much accelerated evolution.

But striking as are the resemblances between these skulls and the early reptilia, comparison with *Megalichthys* shows an equally marked resemblance to the Crossopterygian fish.

The Basisphenoid of *Megalichthys* has sometimes carotid foramina just as in *Loxomma*. It has small but distinct basi-pterygoid processes which are, however, not provided with articulating surfaces but with sutural ones. The long parasphenoid extends forward to the premaxillæ as it may do in *Pteroplax*. Its lateral borders are in contact with the Pterygoids, to which they afford support, and the bone is connected with the roof of the skull by a fused ethmoid.

The Pre-vomer is identical with that of "*Loxomma*" in the majority of its attachments, carries one large tusk and a pit for the replacing tooth. It meets its fellow of the opposite side, and forms the front of the posterior naris; it is doubtful, however, if it meets the palatopterygoid.

The Palatopterygoid of *Megalichthys* is exceedingly like the palatine and pterygoid of *Pteroplax*. They have similar relations to the basisphenoid, parasphenoid and maxilla. There is the same row of small teeth parallel to those of the maxilla with larger teeth inside them, and the pterygoid is covered with the same shagreen of fine teeth.

Examination of these primitive and extremely well-preserved skulls seems to shew that the ordinary idea of the autostylism of the Tetrapoda is incorrect in postulating a connection between the pterygo-quadrate cartilage and the otic region. It is, I think, quite certain that there never was such a connection in primitive forms, except through the dermal bones of the temporal region. The lower attachment with the basisphenoid I have just shown to exist in Crossopterygians, which are hence "amphistylic," in a different way to Notidanus.

The lower jaw of "*Loxomma*" is almost completely known from the material in the Newcastle Museum. The general form was well shewn by Embolton and Atthey, but, as was to be expected from the date of their work, they did not fully understand its structure.

The Dentary meets its fellow in a loose symphysis and extends a long way backwards, ending in a point received in a groove in the outer side of the surangular.

The Splenial (= infradentary) is a comparatively small bone having a small symphysis with its fellow and extending back along the lower edge of the dentary which largely overlaps its outer surface until it terminates by overlapping the angular.

The Angular, as usual, forms the angle of the jaw, the sutures separating it from the surangular and prearticular, meeting low down at the back of the jaw. The surangular is a large bone covering much of the outer side of the articular and running forward, overlapped by the angular, until it is finally cut out by the overlapping dentary meeting the angular. The combined prearticular and coronoid is a very large bone running from the extreme back of the jaw far forward to near the splenial symphysis; towards the front it widens and reaches down nearly to the lower border of the jaw, bordering the tooth-bearing border of the dentary above.

The small bone which in Stegocephalia is usually called the coronoid, but which I have endeavoured to

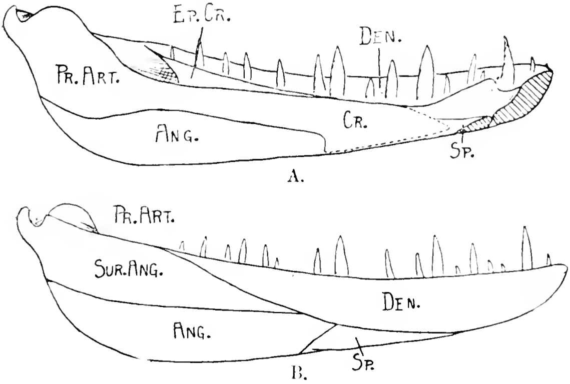

Fig. 3. A. Left ramus of lower jaw of "*Loxomma.*" Inner aspect.
B. Right ramus of the same jaw. Outer aspect. × ¼.
Ang., Angular. Cr., Coronoid. Den., Dentary. Ep.Cr., Epicoronoid. Pr.Art., Pre-articular. Sp., Splenial. Sur.Ang., Surangular.

shew is not homologous with the reptilian bone of that name, and called the Epicoronoid, lies entirely on the inner side of the jaw. It articulates with the dentary above, running along its inner edge for some distance, its lower border being overlapped by the prearticular. The jaw as a whole is of very ordinary type, differing from the majority of Stegocephalian jaws in lacking the internal mandibular vacuity, and being very primitive in the fact that the splenial is entirely a bone of the outer side of the jaw, as is the first infradentary of the Crossopterygian mandible.

The details of the structure of the lower jaw of *Pteroplax* cannot be made out, but it seems to me essentially similar to that of *Loxomma*, modified by the development of two enormous internal vacuities.

Large numbers of vertebræ and ribs occur in the collection, all very similar and all typically embolomerous, the intercentrum being a complete ring nearly as big as the centrum. Typical embolomerous vertebræ are definitely associated with each type of skull. There seems to be little doubt that this type of vertebra is primitive in the large Stegocephalia, the ordinary rachitomous type being almost unknown in the coal measures and becoming commoner in later times.

I wish to express my thanks to the Council of the Northumberland and Durham Natural History Society for the opportunity of describing this important material and to Mr. E. L. Gill, the curator of the Newcastle Museum, for his many kindnesses whilst working on it.

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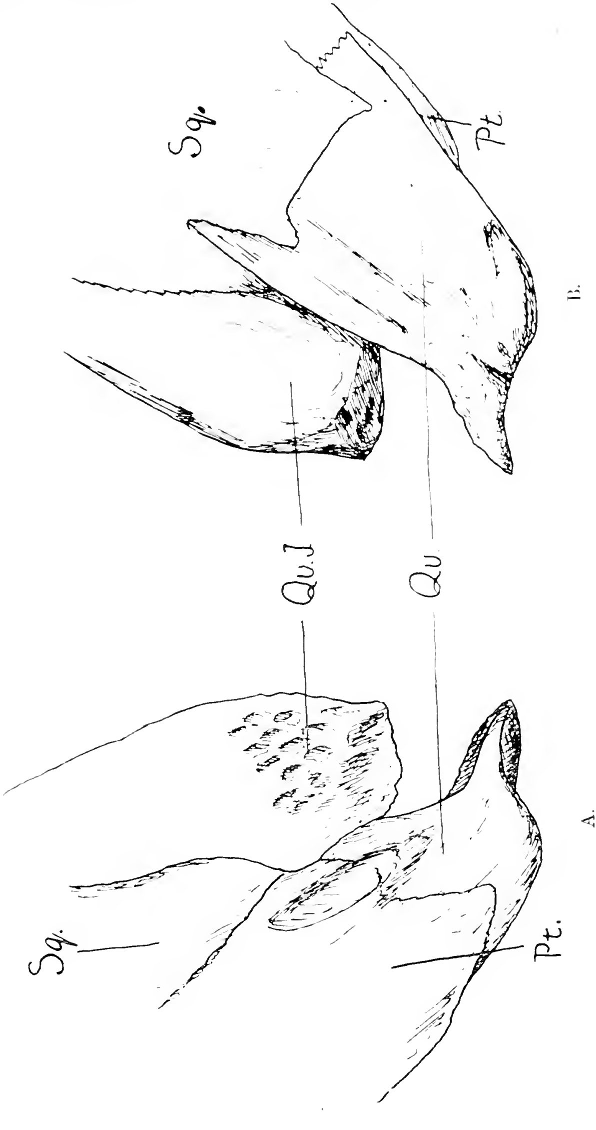
EXPLANATION OF PLATE.

A. "*Loxomma Allmani.*" Right posterior angle of a skull. Dorsal surface.

The Quadratojugal (Qu. J.) and Squamosal (Sq.) are naturally articulated, but have moved forward so as to separate the faces on the Quadrate and Quadratojugal, which are in contact in the undisturbed skull. The Pterygoid (Pt.) and Quadrate (Qu.) are in their correct position.

The ornament on the Squamosal is omitted and that on the Quadratojugal only partly represented.

B. The same specimen as A, ventral surface.


Sq.
Pt.
B.
Qu.J
Qu
Sq.
Pt.
A.

Manchester Memoirs, Vol. lvii. (1912), *No.* 2.

II. On Search-Lights and the "Titanic" Disaster.

By Henry Wilde, D.Sc., D.C.L., F.R.S.

(Received and read November 12th, 1912.)

Following the publication of my paper on "Search-lights for the Mercantile Marine," read before the Society on May 7th last,* formal investigations have been made by the United States Senate Committee and by another Committee appointed by the British Government on the causes leading to the loss of the White Star Steamship "Titanic" on April 15th, 1912. The results of the British investigation have been published as a Parliamentary Blue-book (Cd 6352) dated the 30th of July. The inquiry extended over thirty-seven days, during which ninety-seven witnesses were examined, while a large number of documents, charts and plans were also produced.

The United States Committee examined eighty-two witnesses upon the various phases of the catastrophe, and presented its Report to the Senate, May 28th (No. 806), when it was ordered to be printed. Among other recommendations of this Committee to secure safety of life at sea, prominence is given to the following:—"That every ocean steamship carrying 100 or more passengers be required to carry two electric search-lights."

In the course of the speech of Senator Rayner before the Senate, he reviewed at length the circumstances connected with the "Titanic" disaster and stated that:— "The failure of foreign steamships to carry search-lights is utterly inexcusable, and if a proper search-light had

* *Manchester Memoirs*, vol. 56, 1912.

been on this vessel, in my judgment the accident could have been avoided." The speech was ordered by the Senate to be printed as an annex to the Report and as a public document.

Similar views on the value of search-lights have been expressed by survivors of the disaster; notably by Mr. Beesley, a graduate of Cambridge University and late Science Master at Dulwich College, whose book on "The Loss of the Titanic" has received the highest commendations of the press, "as the only account by a survivor that will ever be published, possessing the importance of an historical document." Under the head of Search-lights Mr. Beesley remarks:—"These seem an absolute necessity, and the wonder is that they have not been fitted before to all ocean liners. Not only are they of use in lighting up the sea a long distance ahead, but as flashlight signals they permit of communication with other ships. He could see through his window as he wrote the flashes from river steamers plying up the Hudson in New York; each with its search-light examining the river, lighting up the bank for hundreds of yards ahead, and bringing every object within its reach into prominence. He supposes there is no question that the collision would have been avoided had a search-light been fitted on the 'Titanic'; the climatic conditions for its use must have been ideal that night."

In a discourse on Icebergs delivered at the Royal Societies' Club on May 9th, Sir Clements Markham, F.R.S., past President of the Royal Geographical Society, stated that during his Arctic explorations the discovery ship had charged an iceberg at 4 knots, stem on, and had been brought up "all standing," but if they had been going at 22 knots he (Sir Clements) would never have been able to tell the tale. The risk for the modern enormous floating hotels might be much reduced by search-lights, and wireless telegraphy had materially added to the chances of safety. Thus he did not think there would be an increase of risk in spite of the enormously increased speed. It was, or had been, the order that ships should not go north of 43 deg. in crossing the 50th meridian, but the "Titanic" had only been in 41 deg. 16 minutes just after crossing that meridian. An international agreement had been suggested that liners of all nations should not go north of a certain latitude; but such an agreement was not likely ever to be made, because the demand for rapid progress and the shortest route was too great. Icebergs had been reported as far south as 38 deg. 40 min. To this I may add that an iceberg has been recorded off the Bermudas Isles, 32 deg. 15 min.

Writing to "The Times" of April 16th, Lord Montagu, who is a noted authority on head lights for motor cars and motor boats, asks:—"Does it not seem curious that powerful search-lights are not habitually used by fast liners during darkness? He submitted that in the case of the fast modern ship one or two powerful head lights are desirable to be used, not spasmodically, but always. Everyone who has seen the ordinary search-lights of the Nåvy at work and stood on the bridge of a ship at night when under way, cannot help having noticed the immense assistance afforded the navigator as regards other shipping, unlighted buoys, or narrow entrances to harbours."

In addition to the foregoing statements by competent observers on the value of search-lights for the mercantile marine, profound dissatisfaction prevails among ocean-going travellers, and officers of the merchant service, in the present state of insecurity of life at sea (especially at night), which finds expression in the press by letters of similar import to those published in "The Manchester Guardian" of the 8th and 19th of August.

With the urgent necessity for restoring the confidence of the public in North Atlantic routes, "The Times" have been asked to state that the Canadian Northern Atlantic mail steamers, "Royal George" and "Royal Edward," are being fitted with powerful search-lights with a range in ordinary circumstances of about two miles; also an improved wireless telegraph service. (*v. Addendum.*)

Dealing now with the Reports of the British official inquiry into the loss of the "Titanic" and of the Merchant Shipping Advisory Committee on life-saving appliances and safety of life at sea. (Cd. 6353) July 24th.

The Report on the loss of the "Titanic," which extends over 74 pages, is remarkable for its brevity respecting search-lights, and consists of five lines only, queried as follows:—"Should search-lights have been provided and used? *Answer:* No, but search-lights may at times be of service. The evidence before the Court does not allow of a more precise answer."

The Report of the Merchant Shipping Advisory Committee of the Board of Trade (175 pages) contains a large amount of valuable statistical information relating to casualties to British ships during the last twenty years in various parts of the world. Of the vessels totally lost, 13 were through striking ice whilst on voyages between European ports and the east coast of the United States, Canada and Newfoundland. In addition to the above casualties there were, during the same twenty years, reported as missing forty-eight vessels registered in the United Kingdom whilst on voyages between the above-named places, with the total loss of 1,083 lives (pp. 59, 137).

The brief statements in the Report on the provision and use of search-lights on large passenger vessels are singularly inaccurate, and biased to a degree that deprives them of all value. The utility of search-lights in picking up rock, land, iceberg, or in passing through a canal is only mentioned as "possible," and ignores the fact that search-lights have been in constant use for many years on the Suez Canal and in the Royal Navy. It is a notable circumstance that these important applications of search-lights find no place in the elaborate Reports of both Committees.

Before the advent of the search-light, all vessels navigating the Suez Canal were required to lay up during the night to avoid grounding and collisions. The navigation is now continuous, and the capacity of the Canal, consequently, nearly doubled.

The first objection in the Report to the use of search-lights is :—"Dazzling the observers on board the ship making use of the lights, especially if the lights are badly placed." The wilful ignorance of those who advanced this objection will be evident to every one who has attended a lantern picture exhibition, and is still more emphatic in the case of a search-light where the beam of light as it issues from the projector is nearly parallel for some distance outside the ship. This is well seen in the annexed photo-plate from the "Illustrated London News" of the search-light on H.M.S. "Agincourt," in the Sea of Marmora, during the Russo-Turkish War, 1878. This battleship was one of those equipped with search-lights under my direction, as referred to in my former paper.

Considerable extensions have been made during recent years in search-light equipment in the Royal Navy; the new flagship "Neptune" having no less than six of them on raised platforms.

The final objection to search-lights in the Report of the Advisory Committee is as follows:—"The disadvantages of search-lights seem to us so greatly to outweigh their advantages that their adoption in the mercantile marine would, in our opinion, be most inadvisable." Now any person of ordinary intelligence may well observe that, if search-lights are indispensable in the Royal Navy, thev are no less so on large passenger vessels. The effrontery of the dominant personalities who are responsible for the above conclusions is fitly comparable with that of the lookout man of the "Titanic," who, to save his credit (as remarked by the President), declared before the Court that the vessel struck the iceberg during a fog, notwithstanding the direct evidence of the officers and other survivors to the contrary.

The statement in the first paragraph of the Report on Search-lights that the Committee have had the assistance of information supplied by the Admiralty is significant in explaining the aberrations of the Committee, and reveals a settled purpose of the Admiralty to monopolise search-lights as an arm in naval operations, regardless of the requirements of the ocean-going public and of the mercantile marine. It will be necessary for Parliament to defeat this object.

That the Advisory Committee and the Admiralty are in close alliance in excluding search-lights from the Merchant Service is abundantly evident from the fact that the whole of the matter contained in the Report of this Committee on Search-lights is copied from the printed evidence of the Assistant Hydrographer to the Admiralty before the "Titanic" Investigation Committee on the 11th of June, two months before the issue of the Report of the Advisory Committee as a Blue-book.

The evasions of the Admiralty witness and his refusal to produce documents when called upon in favour of search-lights in lighting up icebergs were overruled by the Court and the portions withheld were read and printed in evidence. The report of the officer which the witness said "he was instructed not to produce" was to the effect that icebergs and icefields could be made conspicuous at a distance of 2,000 yards, and that when the beam of light struck the ice it looked brilliantly white. The witness also stated, in answer to the President, that the Admiralty had come to the decision that it would be better for ships in the mercantile marine to be without search-lights.

In concluding their Report, the Advisory Committee set forth the importance of securing international uniformity in any new regulations which may be imposed upon the shipping industry, and that any requirements of importance should be enforced on the basis of an international agreement. As the respective views of the American and British Committees on the primary question of Search-lights are absolutely irreconcilable, the diplomatic proposals of the British Committee are not, at the present juncture, to be taken seriously, as they are manifestly put forward to block the way to essential improvements for an indefinite period of time.

In view of the facts brought out by the several Committees engaged in investigating the causes leading to the loss of the "Titanic," it only remains for me to repeat and to emphasise the statement made in my paper read before the Society in May last, that the ultimate responsibility of a calamity which the world deplores rests upon the British naval authorities through their fatuous policy of excluding search-lights from the Mercantile Marine:—The moral forces of the Universe are as real and exacting, for good or for evil, as the physical forces. And "that which men sow that also will they reap."

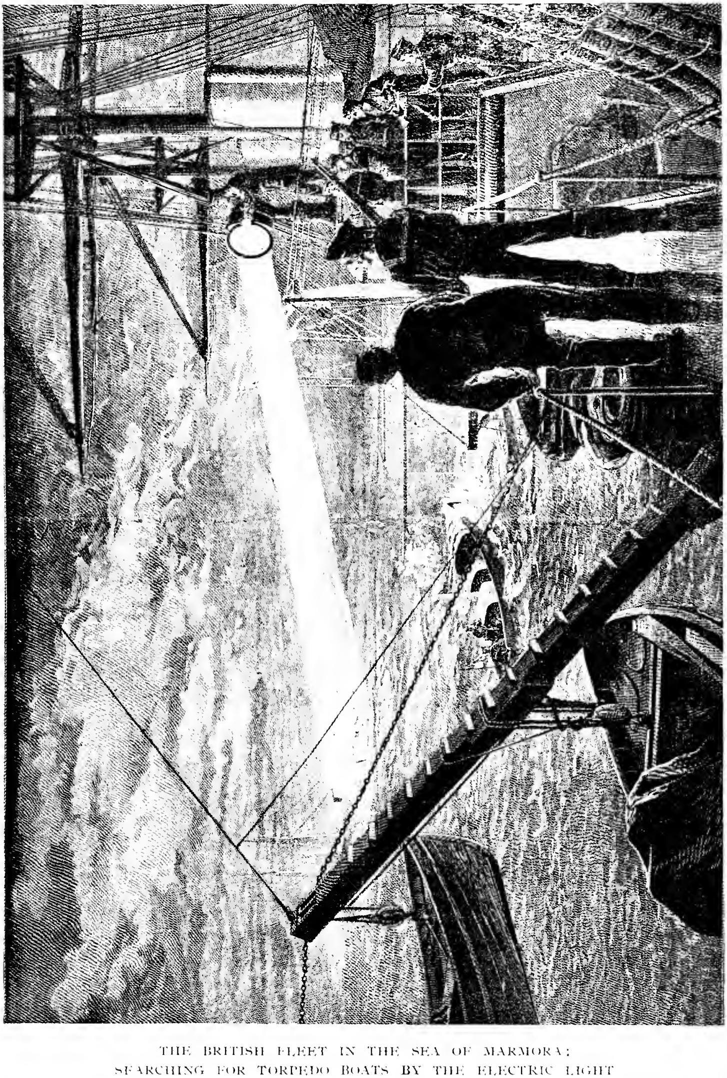
ADDENDUM.

Since the paragraph (page 6) was written on the proposed installation of search-lights on Canadian North Atlantic mail steamers, the "Royal George," with nine hundred passengers on board, ran on the rocks while passing through a narrow channel in the St. Lawrence river, 12 miles below Quebec. The accident occurred late in the afternoon of November 6th, and the captain was maintaining full speed in order to arrive at Quebec before darkness set in, as passengers were not allowed by the port authorities to land at night.

Passengers declared that there was no fog on the river, and were at a loss to understand the cause of the accident. Most of them were rescued with some difficulty up to midnight, and the remainder next day, without any loss of life.

The accident to the "Royal George" is now brought forward as showing the value of search-lights in the landing of passengers arriving at their destination during the night, without the necessity of laying up until next day as formerly in the instance of ships passing through the Suez Canal.

The utility of the electric light projector is much enhanced by the diverging lens: an adjunct for spreading out the beam of light horizontally (*in azimuth*) and lighting up objects at short range. This improvement was devised by me in 1874, at the official request of the Admiralty, and has long been established in the Royal Navy, as will be seen (ready for use) in front of the projector in the *Plate.*



THE BRITISH FLEET IN THE SEA OF MARMORA:
SEARCHING FOR TORPEDO BOATS BY THE ELECTRIC LIGHT
ON BOARD H.M.S. AGINCOURT.

Photo. ILLUSTRATED LONDON NEWS, May 25, 1878.

Manchester Memoirs, Vol. lvii. (1913), No. 3.

III. The Scientific Results of the Salmon Scale Research at Manchester University.

By PHILIPPA C. ESDAILE, M.Sc.,

Research Fellow in Zoology, University of Manchester.

(Communicated by Professor S. J. Hickson, D.Sc., F.R.S.)

(Received and read December 10th, 1912.)

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Research Fellow in Zoology, University of Manchester.

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The study of the fresh-water fauna is at all times most interesting, but the abnormal drought experienced in this country during the summer of the year 1911 greatly increases the interest as well as emphasises the difficulties which are always to be met with when carrying on such work even under normal conditions.

The salmon, living a double life, changing first from fresh to salt water and then back again to the river, cannot but have been influenced by the extraordinary absence of rain, which reduced the rivers to foul and meagre streams, tending to breed disease instead of providing a ready means of access to suitable spawning-beds. Therefore in all studies relating to salmon any abnormal weather conditions must be taken into consideration, and the records obtained during such an abnormal year must not be compared with those of a normal year, unless the differing conditions are kept well in mind. The results obtained from the study of salmon under such unusual weather conditions as were experienced in 1911 may very possibly be most important. Frequently during scientific investigations valuable knowledge can be gained by subjecting the creatures under consideration to different conditions of life, such as placing fish on various kinds of back-

February 6th, 1913.

grounds to observe the colour changes, keeping the animals at temperatures higher or lower than those to which they are accustomed, testing them with different foods, and by many other methods of a similar kind. In the case of the salmon in 1911, Nature herself changed the conditions, and carried out, as it were, a gigantic experiment which man could never have performed himself. Fortunately he can record the results and so take full advantage of the special conditions.

The records of 1911, besides being rendered more interesting on account of the almost unique climatic conditions, are unusual because they are the first of their kind obtained from a series of fish caught during *all* months in the year, permission having been granted for the use of nets for scientific purposes during the close season of 1911-1912.

The scales were all taken from the "shoulder" of Wye salmon caught either by nets or by rods. These scales were examined and the fish classified according to the number of years spent in the sea (See Table 1). In a previous paper (1) similar results were given for salmon caught in the Wye during the years 1908, 1909, and 1910. A comparison of the combined results of those years, with the results of 1911 (See *Graphs* 1 and 2), indicates that there were considerable differences in the percentages of the various classes of fish caught at different periods of the year. The explanation of these variations is difficult to ascertain as it is uncertain whether any can be given until much more is known about the distribution and food of the salmon while in the sea. Records of a large number of fish have so far been taken systematically for only two years, 1910 and 1911, and we are therefore not in a position to state what may be considered to be the normal conditions. Many mistakes might be made if we stated that this or that result was normal. This paper can only record differences in the results of the two years, the full meaning of which cannot at present be understood.

The results obtained in 1911 are very similar to those of 1910 in one respect (see Tables 2 and 3). The majority of the fish, 94·69% (see Table 3) spent only 2 or a little more than 2 years (= 2 +) in the river before going down to the sea. In 1910 the percentage was slightly lower, 91·7% (see Table 2), but this small difference is of little importance. Of the remaining 5·31% (in 1911) it is interesting to notice that slightly more than half (2·84%) lived in the river for three years, while the rest spent less than 2 years in the river. These proportions roughly correspond with those obtained for the 1910 fish. Among the fish which spent less than 2 years in the river (see Table 5), by far the greater number remained in the sea for 3 or more years, and only 22·5% returned to the river after remaining in the sea for less than 3 years. When we come to consider the fish which spent 3 or 3 + years in the river the percentages are reversed, *i.e.*, the majority remained less than 3 years in the sea. The greater number of those fish which were 2 or 2 + years old at the time of migration returned to the river after an absence of less than 3 years.

The important point to be noticed in these figures is that as a rule the fish which lingers in the river for a considerable time spends a briefer period in the sea before returning to spawn than a fish which had a short life in the river as a parr. It is true that up to December, 1911, no more than 100 fish were examined from the Wye which had spent either 1 and 1+ or 3 and 3+ years in the river; that is to say, decidedly more or less than the average. This means that only 5·5% were exceptions to what appears to be the normal type characteristic of the Wye. In other words, the characteristic type of fish that occurs in the Wye is one which spends two years in the river, but 5·5% were proved to be exceptional in this respect.

From these figures it seems that among the Wye salmon there is a fairly definite age at which they reach sexual maturity; that is, between four and five years. The majority of the Wye salmon spend two years in the river, and by far the greater number of these, as has been stated above, return to the river after an absence of less than 3 years. Of the remaining 5·5% those which stay in the river for a considerable period return sooner to the river, and are generally about the same age at the time of spawning as those fish which remain only a short while in the river and a longer time in the sea. All this, therefore, points to the fact that there is a definite age at which spawning generally takes place and that the length of time spent in the river determines the length of time spent in the sea.

Bearing upon this point Herr Knut Dahl brought forward some very interesting facts in his recent paper (2), and they are of great use when considering fish from other localities. With regard to the relative length of time spent in the river and sea, Herr Dahl shows that the salmon from Finmark, in the north of Norway, remain in the river for a much longer time than the fish from the more southerly districts of Trondhjem and Christiansand. The results indicate that the majority of the fish from Christiansand remain in the river before migration for 3 years, while the greater number of fish from Finmark spend 4 or 5 years in the river. Comparing these results with those from the Wye, which is about 7·5° further south than Christiansand, we find that two years is the average length of time spent in the river. From time to time scales of fish caught in many different localities have been sent to the Manchester University for examination. Several of these fish were from the Hampshire Avon, the Dorsetshire Stour (3), and some were from La Creuse (4) and La Vienne. Although specimens of scales were not collected from a sufficient number to make it worth while to quote figures, it is significant that almost without exception the fish had remained in the river for only one year. Scales of salmon taken in the Hampshire Avon have been examined by Mr. Johnston (5), and he records similar results.

This regular series of decreasing lengths of time spent in the river noticed in fish from Finmark, Trondhjem, the Wye and the Avon, Stour, Vienne and Creuse raises a very interesting point. What influences the length of time spent in the river? From the results mentioned above it would naturally be supposed that the temperature of the rivers must be an important factor. There are no data to hand of the temperatures of the rivers in the localities named, but from their geographical positions it would be safe to say that the temperature of the Stour, Avon and Vienne would be higher on the whole than that of the rivers of Finmark. It would therefore seem that the temperature of the water may have a considerable influence on the length of time the fish remain in the river. With the higher temperature there might be quicker growth and possibly a more rapid approach to the stage at which the young salmon are ready to go to the sea. In order to settle this point satisfactorily, careful investigations should be made in different localities, ascertaining the average size of the young salmon at the time of migration and the average size of the fish on their return to the river to spawn. We should then fully understand whether the young salmon in the north of Norway leave the rivers when approximately the same size as the young salmon in the Wye. It might be that those from Finmark are typically larger than those of the Wye, thus necessitating a longer life in the river.

There is rather an interesting difference between the records of the two years 1910 and 1911; the percentage of fish which spent 2 and 2+ years in the river and only 1+, 2 or 2+ years in the sea is much higher in 1911 than in 1910. It will be remembered that in 1911 records were obtained from fish caught in all months of the year, during both the fishing season and the close season, while in 1910 records were taken during the fishing season only. However, even when the records obtained during the close season of 1911 are not included, there is still a great difference in the percentages, 73% in 1911 and 57% in 1910. Taking into account the drought of the summer of 1911, it would have been natural to reason that the fish delayed their return to the fresh water, and in this case the percentage of 1911 should have been lower than that of 1910. However, the excessive heat which accompanied the drought of the summer of 1911 may possibly have influenced the conditions of life of the salmon while in the sea and caused a hastening of sexual maturity. This is again a question which can only be settled by further research, and a comparison of temperature and percentages, etc., of fish caught in various years would be necessary for this purpose.

When first this work was undertaken it was intended that a most careful examination of the scales should be made with a view to ascertain the relative number of annuli present in the peronidia of river and sea life. This has, however, been abandoned, as it would have delayed the publication of any statement about the research for at least two years. Quoting from my previous paper (6), the definitions of the terms peronidium and annulus were as follows:—

"On closer examination it will be found that the inequalities (on the external surface of the scales) are caused by what have hitherto been called 'lines' or 'ridges,' but which I shall term annuli (see photo of scale *A*). These annuli are arranged in a roughly concentric manner from the centrum of the scale (*C*). . . . Examination with the low power of the microscope shows that the annuli are arranged in a definite manner, some far apart and others close together. Those far apart are, according to Mr. Johnston, formed during the rapid growth of the fish in the summer, and those closer together during a time of slow increase in the winter. This formation of annuli far apart, together with the formation outside this of annuli more closely placed, was called by Mr. Johnston an 'Annual ring.' For this name the word 'Peronidium' is substituted, and it is used to indicate the growth which takes place in a complete summer and winter."

Unfortunately I overlooked a paper by Professor T. D. A. Cockerell, published early in 1911 (7), in which he also gives terms for the different parts of the scales.

"The key to the origin of the sculpture of a teleostean scale is apparently to be found in that ancient type the *Amia calva* of North America. Fig. 1 shows part of the base of the scale of this fish, which, it will be observed, consists of longitudinal strands or fibres, separable elements which fray out basally. In the apical field these are directed towards a rough nucleus area. A close approximation to this is found in a very old type of teleosteans, the lady-fish *Albula*. In this, however, appear also the beginnings of the radial lines, extending from the nucleus of the scale to the margin. As we go higher in the scale of fish evolution, these radiating lines or *radii* often become very prominent, while the longitudinal strands usually become united above and below, forming circular fibres which we have designated *circuli*. The nomenclature of these structures was based on a normal highly-developed scale, in which the circuli deserved their name, and since then the term has been applied to the same elements wherever found, so that I have had to refer, rather illogically, to *longitudinal circuli.* Perhaps it would be better to call them 'fibrillæ.'"

Mr. J. A. Hutton, in *The Field* (8), puts forward a list of English names for the different parts of the scale. However, I propose to keep to the terms "centrum," "annulus" and "peronidium." They have been clearly defined, and by their right use no misunderstanding can occur. It should also be remembered that terms such as those I have put forward are capable of being adopted and understood by students of all nationalities whereas English words have not this advantage, and might be misinterpreted by translation.

It has been suggested that if there is a true racial difference between the spring and summer fish some indication of this might be shown by the average number of annuli in the peronidia of the two kinds of fish. Such a suggestion, however, does not appear to be supported by the results that have, up to the present date, been ascertained.

A careful examination of a large number of scales taken from different parts of the same salmon proved that the number of annuli in corresponding peronidia exhibit considerable variation (6). No two scales from the same fish appear to be exactly the same in this respect although there is a greater similarity between scales taken from the same part (*e.g.*, the shoulder or the tail, etc.) than there is between scales taken from different parts.

To prove, therefore, that there is a racial difference, as regards the scales, between the spring and summer fish it would be necessary to compare the average number of annuli in the peronidia of a large series of scales taken from exactly the same part (*e.g.*, the shoulder) of fish of the two kinds. If such a comparison proved that there is a well-marked difference between the scales of the two kinds of fish, and that this difference is greater than the known difference between scales of the same region in the same fish and greater than a difference due to error of observation, there would be some reason for believing that the two kinds of fish are distinct races.

Such observations have not been made on an extensive plan, but the researches carried on in the Manchester University during the past two years are sufficient to prove that such a distinction between the two kinds of fish, if it exists, must be so slight that it can be of very little practical use.

It should be stated quite clearly, however, that the results obtained do not disprove the theory that there are two races of salmon. All that is shown is that the difference between the two races is not clearly marked by a difference in the number of the annuli in the scales.

In spite of the fact that the data as regards the scales do not aid in a consideration of the racial question, there are many very interesting points which can be learnt from them. For instance, if we classify the fish according to the length of time spent in the sea, comparisons can be made between the relative weights and lengths and girths of what have been termed the various kinds of salmon. (See Table 6.) It will be observed that the average weight of the small spring fish is very nearly equal to the average weight of the small summer fish, and the same is true of the large spring and large summer fish. This brings forward the question whether these fish have lengths and girths corresponding to their weights. To answer this the ratios of length and weight and length and girth have been ascertained for each kind of fish and their averages compared. Considering first the averages of the ratios of length and weight, *Graphs* 3 and 7 indicate that on the whole the fish which have spent a long time in the sea are heavier for their length than those which spent only a short time in the salt water. From this it might be expected that the curves of the averages of the ratios of length and girth would correspond with those of the averages of the ratios of length and weight. However, the *Graphs* 5 and 9 indicate that this is not the case; the curves expressing the averages of the ratios of lengths and girths do not show this decided decrease in the case of those fish which have remained in the sea for three or more years. We find, therefore, that whereas the fish become heavier in relation to their lengths the longer they remained in the sea, this increase in weight is not accompanied by a corresponding increase in girth.

Passing on to a consideration of the amount of variation from the averages of the ratios of the different kinds of fish there are some interesting results. In order to make a correct comparison the coefficients of variation have been used for this purpose.

Pearson's formula for the calculation of the coefficients of variation is as follows:—

$$\text{Coefficient of variation} = \frac{\sqrt{\frac{\Sigma x^2}{n}}}{M} \quad \text{where } M = \text{mean},$$

Σx^2 = the sum of the squares of the deviations from the average, n = the number of specimens examined.

Graphs 4, 6, 8, and 10 indicate a considerable amount of variation. In the results for 1911 there seems to be some definite connection between the averages of the ratios of length and weight and length and girth and their coefficients of variation. This is to be noticed in the similarity of the curves in the graphs; the coefficients of variation are higher where, in the corresponding kind, there is a high average for the ratios, and, on the other hand, where there is a low average there is a low coefficient. This shows clearly that with an increase in length for weight and length for girth there is a corresponding increase in variability. In other words, the grilse and small spring fish are more variable in these repects than larger fish. The records for 1910 do not show this regularity of correspondence, but it must be borne in mind that in the autumn of 1910 netting did not take place in the close season, and it is during this time of year that we should expect to find specimens that would raise the coefficient of variation for the grilse.

Comparing *Graphs* 3 and 7, which give the curves representing the averages of the lengths and weights of the different kinds of salmon taken in 1910 and 1911, it is to be observed that the two curves are of different types. *Graph* 3 shows a practically smooth curve while *Graph* 7 gives a curve with three maxima, recording respectively the results obtained from the grilse and from the small and large spring fish. This variation, however, can perhaps be accounted for by the very long continued drought of 1911. After April there was very little water in the Wye, and the grilse and summer fish which, in the natural course of things, should have run up the river in June were unable to get over the weirs and were therefore caught in great numbers by the nets at the mouth of the river. The scales of these fish—in fact, of nearly all kinds of fish caught after the beginning of May—show disintegration to a greater or less extent. This, so far as we understand at present, means that the sea-feeding has ceased and that the long fast previous to spawning has begun. It is most interesting that fish with scales of this type should be caught in the nets, and we can only suppose that these salmon were waiting for a flood to make their way up to the spawning beds. In other words, the grilse and summer fish appear to have been waiting about at the mouth of the river, and having begun the fast previous to spawning were living on their adipose tissue. Their weight, therefore, was becoming reduced, and as the length could not become less, the ratio of length and weight would naturally be higher, and hence the maxima observed in the curves for 1911.

After consultation with mathematicians, I find that the records were not taken with sufficient accuracy to render the second place of decimals absolutely reliable. However, I have not left it out, as it would only mean a modification of the curves and would not materially change their direction.

Coming next to the consideration of the scales of those fish which are on their second visit to the river and have spawned in a previous season, we are confronted with, probably, the most difficult, but, at the same time, the most interesting part of the whole question of the life-history of the salmon. The first point which should be noticed is the small proportion of fish that have previously spawned. This is, however, a fact which is by no means new, previous workers having commented upon the infrequency of spawning in the salmon. Calderwood (9), (10), (11), Hutton (12, (13), Esdaile (1).

It is now generally accepted that the salmon does not take any regular nourishment during its sojourn in the rivers previous to spawning, and this fact in itself may afford one very good explanation for the infrequency o spawning. As a general rule, when there are long periods of fasting during the life of an animal there is hibernation, or at any rate a prolonged sleep or state of quiescence. But in the salmon the period of fasting is probably the most strenuous time of its life. Every time this problem is discussed new evidence is accumulated to prove that the spawning salmon are of much more importance for the fishery than was realised some little time ago. The old idea that the salmon returned to the river year after year and was the dutiful and self-sacrificing parent of many broods of eggs is now shown to be quite erroneous. It is therefore all the more important that fish, when they do come to spawn, should be protected in every way, and that the young fry, when hatched, should be provided with the best possible environment.

The proportion of fish showing a spawning mark was practically the same for the two years 1910 and 1911. The total number of fish examined was 1678, and of these only 78 showed a previous spawning mark. If we exclude from this total number of fish examined all grilse and small spring fish we find that of the remainder—that is the large fish found in the river—only 6·4% have previously spawned.

Out of the 78 fish only five have two spawning marks; that is to say, only five salmon have been able to spawn twice and live to enter the river for the third time. The results obtained from these fish will be considered later, as they are more readily understood after the records of the scales with one spawning mark have been discussed.

The scales offer peculiar advantages to the student; they show the life-history of each particular fish, and in the case of scales bearing a spawning mark they tell the story of a comparatively long and eventful life, the wanderings of great travellers. They are, as it were, the diaries in which we read of the incidents of the journey, the approximate time of arrival and departure at the various stages, and from which we can judge to a certain extent the effect of the journey upon the fish.

It should be specially noted that the scales of fish which have previously spawned are extremely difficult to read. During the long fast the periphery of the scales becomes disintegrated and an appearance of fraying is produced. When the disintegration has been extensive it is often very puzzling to determine whether the fish entered the river as a spring or later autumn fish. However, after some experience, this work can be carried on with sufficient accuracy, especially when many scales are examined from each fish, to warrant a consideration of the results. It might be mentioned here that the results obtained by the Scottish and Irish Fishery Boards from the scales of marked fish are very similar to the results obtained by the examination of the scales of unmarked Wye fish. Hillas (14), Calderwood (15) (16).

The ages at which these fish returned to the river previous to the first time of spawning range from 3 + to 5 + years (see Table 7). The majority of them entered the river for the first time as grilse and summer fish. Dividing the fish into two groups 51 spawned for the first time as grilse or summer fish and only 22 as spring fish. When the whole circumstances are taken into consideration a proportion of this kind is only what might be expected. The "fresh" spring fish entering the river in December, January or February have already ceased feeding, and up to the time of their return to the sea, that is, if they are so fortunate as to be able to return, no regular nourishment is taken. This means that for twelve or thirteen months these creatures are living a most exhausting life, giving out a tremendous amount of energy in their efforts to reach the spawning beds, and in addition to this there is a considerable drain on the whole system, due to the development of the sperms or ova. It has often been observed that the testes and ovaries of the early spring fish are very small and immature, which means that the greater part of their development takes place during the long period of fasting. The summer fish, on the other hand, are feeding in the sea during the spring and early summer months, and the scales seem to indicate that in some cases feeding may be continued even up to September. There is a theory that those fish which enter the river later in the year do not make their way up to the highest "redds" but spawn much nearer to the sea. According to this the summer fish has not such a long time of fasting, neither has it so far to travel during the fast. Remembering all these circumstances, it is not astonishing to find that so few spring fish survive to spawn a second time.

It is true that the spring fish, at the time they enter the river, are in much better condition than the summer fish, but the study of scales clearly shows that the sojourn in the river is more fatal to the former than it is to the latter.

A practical result that follows from this consideration is that excessive fishing in the summer is slightly more harmful than excessive fishing in the spring, because the chances that the fish caught would have returned a second time to spawn are greater in the case of the summer fish than they are in the case of the spring fish.

As bearing to some extent on the question of the racial differences in salmon, it is interesting to turn now to the consideration of the time of year when the fish return to the river to spawn for the second time. If there are two distinct races of salmon, the spring race and the summer race, it might be expected that the instinct to return to the river would be felt at the same time of year in successive spawning efforts. We should expect the spring fish to come up to spawn in the spring and the summer fish in the summer. But there seems to be no such law governing the return of the fish to the river (see Table 9).

There were in the series examined twenty-two spring fish, and of these nineteen returned to the river to spawn for the second time in the spring, and there were fifty-one summer fish, and of these only fifteen returned for the second time in the summer. It is evident, therefore, that in a considerable number of cases the spring fish may come back again in the summer and the summer fish come back in the spring.

Passing on to compare the lengths of time spent in the sea between the return from the river after the first spawning and the second return to the river previous to the second spawning, there is again no definite regularity. (See Tables 8 and 9.) All the "spring summer" fish, as they might be called, spent only a few months in the sea between the two spawning periods and the "spring spring" fish and "summer spring" fish spent one year in the sea. Among the "summer summer" fish there are examples of two periods of differing lengths, and the three "grilse summer" fish show as many variations as there are specimens.

Lastly, there is the consideration of the five fish which were caught on their way up the river to spawn for the third time. Four of these belong to the same type. At each return to the river in three consecutive years they were summer fish, and they only spent seven or eight months in the sea between each return to the river. The fifth specimen spawned first as a spring fish, then as a summer fish, and was finally caught as a spring fish on its third return to the river. Between the first and second spawning the fish was in the sea for only seven or eight months, while between the second and third return about fifteen months were spent in the sea. It is rather curious to notice that this corresponds with the sequence which is indicated in Table 9, "spring," "summer," with a few months spent in the sea between the first and second return to the river, and "summer," "spring," with a year spent in the sea between the second and third return to the river.

From Table 8 it would appear that contrary to what would have been expected the salmon is not generally an annual spawner. Only 12 specimens out of 73 made the attempt to spawn in two consecutive years, while 55 returned to spawn the second time in the year but one following that in which they spawned for the first time.

Conclusions.

Up to the present time all the investigations with regard to the life-history of the salmon have been of very great interest on account of the many unexpected results which have been obtained. The collection of scales under discussion appears to present quite a number of unlooked-for irregularities. The fish, according to the scales, remained in the sea for various lengths of time previous to spawning. There appears to be no rule which determines the length of stay in the sea between the first and second spawning periods. In fact, so far as the results go, it would seem that there is no definite law governing the migrations of the salmon ; each fish is a law unto itself, living its own independent life, travelling about when it pleases. From a research of this kind, the "shoal" of salmon entering a river is put before us in quite a different light. From these results it can be no longer considered to consist of a number of fish of similar ages which have lived side by side all their lives. The shoal more closely resembles a crowd of people entering a concert hall. In each case the aim in view appears to be the only thing in common ; the salmon of varying ages enter the river to spawn just as the people of various kinds crowd into the hall to hear the concert.

During the past two or three years many doubts have arisen with regard to the salmon, and theories have been formed in the endeavour to explain the very striking differences between what are known as "spring" and "summer" fish. One of these theories is that the "spring" and "summer" fish represent two distinct races; this would mean that spring fish bred spring fish, and summer fish summer fish. However, the scales which have been examined at the Manchester University do not appear to lend any support to this theory. It is to other data that we must now turn to settle the question satisfactorily; such as the number of vertebrae, the number of fin rays present in the various fish, or the form of the bones might perhaps show some distinguishing features which would definitely decide the matter.

The fish that have spawned twice give even more opposition to any such theory. When a fish is first a spring fish, then a summer fish, and is finally caught in the spring, the time of year would appear to be of no importance as an indication of racial differences among salmon.

It should be noticed that there are no data such as were used by Professor Garstang (17) in his paper on "Variations and Races of Mackerel," such as the number of rays present in the various fins of fish from different localities. Neither have we the accurate measurements (of the sizes and distances of the various fins, etc.) similar to those taken by Dr. Jenkinson in his recent paper (18) on "Growth, Variability and Correlation in Young Trout." The available data only record fluctuations, due in all probability to the amount of food taken and to the particular circumstances under which each fish has spent its life. These data are of no use when we come to consider whether the various runs of salmon are all of one race or whether there are two distinct races which might be characterised as "spring" and "summer" fish.

In a work of this kind enough precaution can scarcely be taken against generalising from few data. The records used in this paper were obtained from only one river and from only a very small percentage of the total number of salmon in that river. With this reservation this paper must be looked upon as a means to an end and not as a final conclusion of any part of this vast subject.

Summary.

I. There appears to be some relation between the length of time spent in the river and in the sea, *i.e.*, when a young fish lingers in the river for a considerable time it remains in the sea for a comparatively short period and *vice versa*.

II. This seems to indicate that among the salmon in the Wye, at any rate, there appears to be a more or less definite age for sexual maturity.

III. The temperature of the water may possibly be one of the determining factors of the length of time spent in the river before migration.

IV. Those salmon which have a short sea-life are longer for their weight and show more variation in their measurements of lengths, girths and weights than those remaining in the sea for some time.

V. The infrequency of spawning of the salmon. The spawning period appears to be more fatal to "spring" than "summer" salmon.

VI. There appears to be no definite law governing the migrations of the salmon. The time of year at which a fish enters the river to spawn appears to be of no importance as an indication of racial differences among salmon.

VII. The results indicate that the salmon is not as a rule an annual spawner, but in the majority of cases, where the salmon does return to spawn for the second time, it is in the year but one following that in which it first spawned.

The research was conducted in the Manchester University with the aid of a grant that was kindly provided by Messrs. J. A. Hutton, H. Behrens, and R. Beddington. I am also indebted to these gentlemen for providing all the material on which the research was conducted.

My thanks are due to Professor S. J. Hickson, under whose direction the work has been carried on, and who has been most untiring in giving me help and advice. I wish also to thank Dr. J. Stuart Thomson for all the help he has so readily given me.

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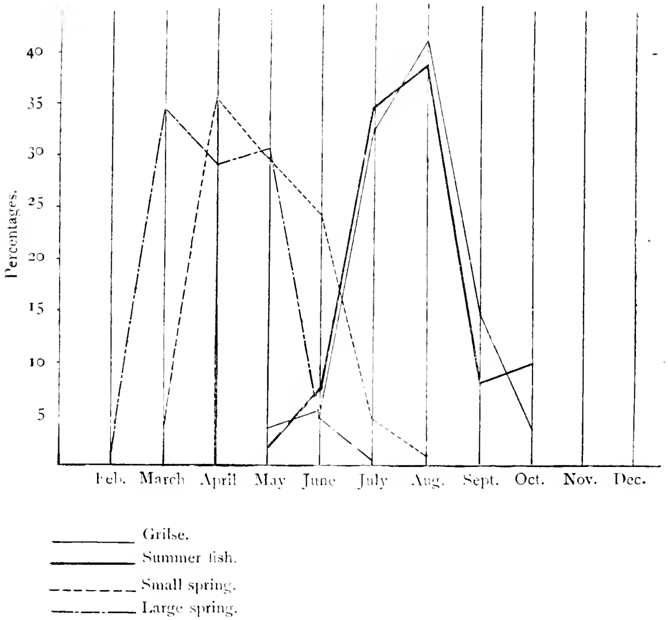
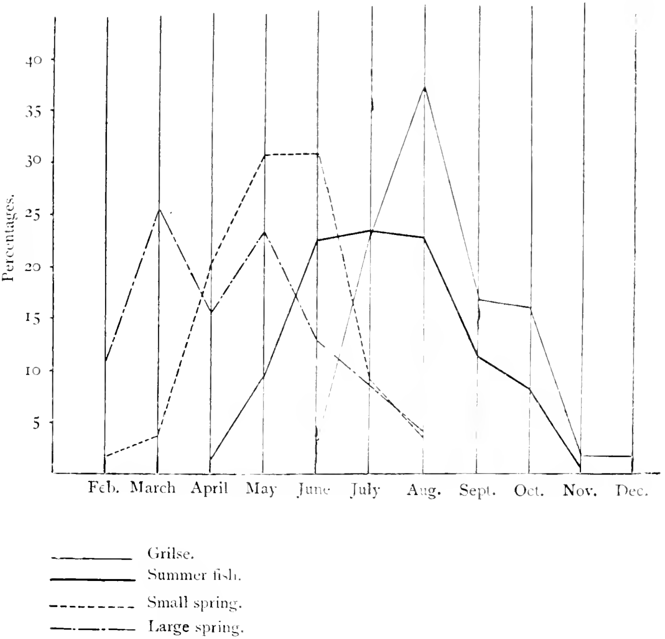
GRAPH I.

PERCENTAGES OF EACH KIND OF SALMON CAUGHT IN THE VARIOUS MONTHS, 1908, 1909 AND 1910.



GRAPH 2.

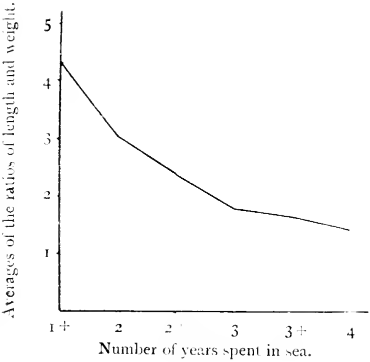
PERCENTAGES OF EACH KIND OF SALMON CAUGHT IN THE VARIOUS MONTHS, 1911.



RESULTS FOR 1910.

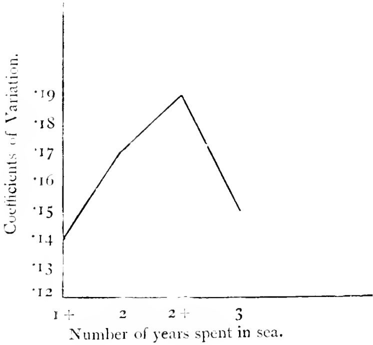
Comparing the averages of the Ratios of lengths and weights of the different kinds of fish.

GRAPH 3.



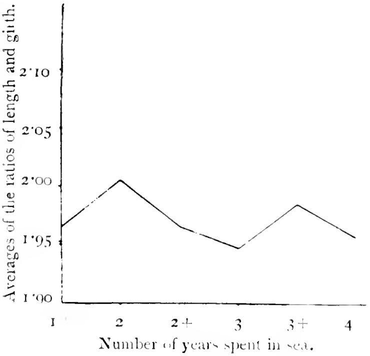
Correlation of the Coefficients of Variation of lengths and weights.

GRAPH 4.



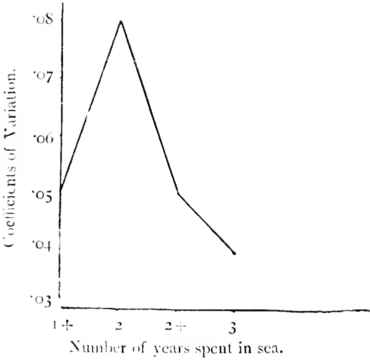
Comparing the averages of the Ratios of lengths and girths of the different kinds of fish.

GRAPH 5.



Correlation of the Coefficients of Variation of lengths and girths.

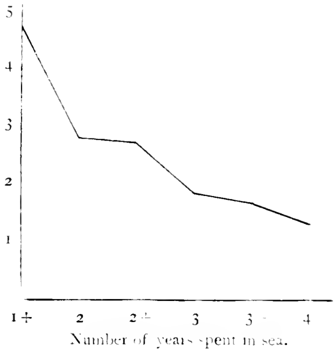
GRAPH 6.



Results for 1911.

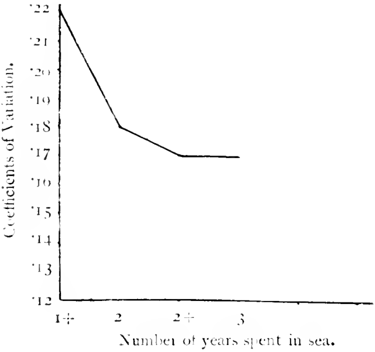
Comparing the averages of the Ratios of lengths and weights of the different classes of fish

Graph 7.



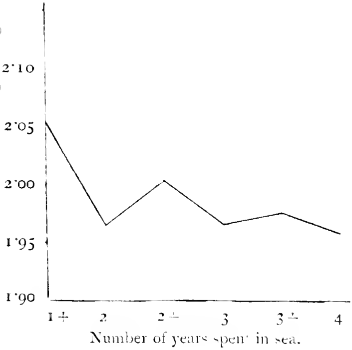
Correlation of the Coefficients of Variation of lengths and weights.

Graph 8.



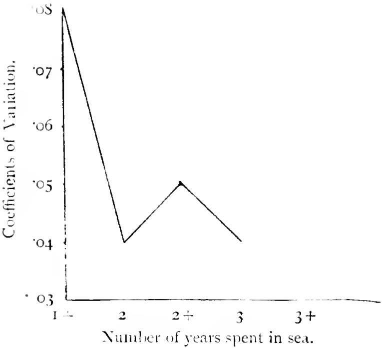
Comparing the averages of the Ratios of lengths and girths of the different classes of fish.

Graph 9.



Correlation of the Coefficients of Variation of lengths and girths.

Graph 10.



LIST OF TABLES.

TABLE I.

NUMBER OF SALMON OF EACH KIND CAUGHT DURING 1911.

NUMBER OF YEARS SPENT IN THE SEA SUBSEQUENT TO MIGRATION AS SMOLTS.

	Nets.									Rods.									Total.							
	1+	2	2+	3	3+	4	5	Sp.	Total.	1+	2	2+	3	3+	4	5	Sp.	Total.	1+	2	2+	3	3+	4	Sp.	Total.
February.....	—	3	—	16	—	—	—	3	22	—	—	—	11	1	2	—	1	15	—	3	—	27	1	2	4	37
March..	—	4	—	12	—	—	—	3	19	—	3	—	50	—	4	—	4	61	—	7	—	62	—	4	7	80
April	—	39	5	26	—	—	—	9	79	—	1	1	14	—	1	—	2	19	—	40	6	40	—	1	11	98
May.....	—	54	40	43	—	2	—	11	150	—	6	6	16	—	3	—	5	30	—	60	46	59	—	5	16	180
June	—	56	109	31	5	1	—	3	205	3	4	8	2	—	—	—	1	18	3	60	117	33	5	1	4	223
July...........	25	18	120	21	2	—	—	3	189	1	—	1	1	—	—	—	—	3	26	18	121	22	2	—	3	192
August	41	7	118	10	—	—	—	2	178	1	—	—	—	—	—	—	—	1	42	7	118	10	—	—	2	179
September ..	19	—	60	—	2	—	—	8	89	—	—	—	1	—	—	—	—	1	19	—	60	1	2	—	8	90
October	18	—	44	—	1	—	—	—	63	—	—	—	—	—	—	—	—	—	18	—	44	—	1	—	—	63
November ..	2	—	3	—	—	—	—	—	5	—	—	—	—	—	—	—	—	—	2	—	3	—	—	—	—	5
December ...	3	—	1	—	—	—	—	—	4	—	—	—	—	—	—	—	—	—	2	—	1	—	—	—	1	4
	107	181	500	159	10	3	—	43	1,003	5	14	16	95	1	10	—	13	154	112	195	516	254	11	13	56	1,157

TABLE 2.

COMPARISON OF THE NUMBER OF YEARS SPENT IN THE RIVER BEFORE MIGRATION AND THE LENGTH OF TIME SPENT IN THE SEA BEFORE RETURNING TO THE RIVER BY SALMON CAUGHT IN THE WYE DURING THE YEARS 1908, 1909 AND 1910.

	NUMBER OF YEARS SPENT IN THE RIVER BEFORE MIGRATION.											
Number of years spent in the sea.	1	2	3	Total.	1+	2+	3+	Total.	1 & 1+	2 & 2+	3 & 3+	Total.
1 +	—	8·2	0·85	9·05	—	4·05	—	4·05	—	12·25	0·85	13·1
2	0·15	12·3	1·90	14·35	0·3	5·4	—	5·7	0·45	17·70	1·90	20·05
2 +	0·90	13·25	0·15	14·30	—	9·2	—	9·2	0·90	22·45	0·15	23·5
3	1·35	20·15	1·3	22·80	0·3	14·7	0·4	15·4	1·65	34·85	1·70	38·2
3 +	0·15	1·0	0·4	1·55	—	0·7	—	0·7	0·15	1·70	0·40	2·25
4	—	1·5	—	1·5	—	0·85	0·15	1·0	—	2·35	0·15	2·5
5	—	0·4	—	0·4	—	—	—	—	—	0·40	—	0·4
Total	2·55	56·8	4·6	63·95	0·6	34·6	0·55	36·05	3·15	91·70	5·15	100·00

TABLE 3.

PERCENTAGES OF THE COMPARISON OF THE LENGTHS OF TIME SPENT IN THE RIVER AND SEA, 1911.

NUMBER OF YEARS SPENT IN RIVER BEFORE MIGRATION.

Number of years spent in sea.	1	2	3	Total.	1+	2+	3+	Total.	1 & 1+	2 & 2+	3 & 3+	Total.
1 +	—	7·94	·73	8·67	—	1·37	—	1·37	—	9·31	·73	10·04
2	·09	13·15	·46	13·70	·09	3·93	—	4·02	·18	16·08	·46	17·72
2 +	·28	36·50	·92	37·70	·09	9·13	·09	9·31	·37	45·63	1·01	47·01
3	·64	16·61	·55	17·80	1·09	4·11	—	5·20	1·73	20·72	·55	23·00
3 +	·19	·74	·09	1·02	—	·28	—	·28	·19	1·02	·09	1·30
4	—	·74	—	·74	—	·19	—	·19	—	·93	—	·93
Total	1·20	75·68	2·75	79·63	1·27	19·01	·09	20·37	2·47	94·69	2·84	100·00

TABLE 4.

1910.

Years spent in the river.	Percentage of fish which returned to the river after	
	1 +, 2 or 2 + years of sea-life.	3, 3 + or 4 years of sea-life.
1 and 1 + ...	40%	60%
2 and 2 + ...	57%	43%
3 and 3 + ...	63	37%

TABLE 5.

1911.

Years spent in the river.	Percentage of Fish which returned after	
	1 +, 2 or 2 + years of sea-life.	3, 3 + or 4 years of sea life.
1 and 1 + ...	22·5	77·5
2 and 2 + ...	76·2	23·8
3 and 3 + ...	77·4	22·6

Table 6.

Table giving the average weights, lengths and girths of the Various Kinds of Salmon, 1911.

Class of fish.	Number of years spent in sea.	Averages.		
		Weight in pounds.	Length in inches.	Girth in inches.
Grilse	1 +	5·3	25·9	11·9
Small spring ...	2	10·2	30·4	15·3
Small summer...	2 +	11·7	32·1	13·0
Large spring ...	3	20·5	37·7	19·2
Large summer...	3 +	22·5	39·9	19·9
Very large spring	4	33·0	44·8	21·6

Table 7.

Age of fish (in years) entering the river previous to spawning for the first time.

	3 +	4	4 +	5	5 +
Number of fish	6	10	43	12	2

(All these fish were caught when on their second return to the river and their scales therefore show spawning marks.)

Table 8.

Length of time spent in sea after 1st and before 2nd spawning.

	A few months.	1 year.	1 + years.	2 + years.
Number of specimens	12	55	5	1

Table 9.

No. of fish.	Type at 1st spawning.	Type at 2nd spawning.	No. of fish.	Length of time spent in sea between 1st and 2nd spawning.
3	spring	summer	3	a few months.
19	spring	spring	19	one year.
36	summer	spring	36	one year.
12	summer	summer	8	a few months.
			4	one year + a few months.
3	grilse	summer	1	a few months.
			1	one year + a few months.
			1	two years + a few months.

Manchester Memoirs, Vol. lvii. (1913), *No.* 4.

IV. Note on the Mean Magnetic Moment and Mean Energy of a Vibrating Magnet.

By J. R. ASHWORTH, D.Sc.

(Communicated by Mr. R. L. Taylor, F.C.S., F.I.C.)

(Read December 10th, 1912. Received for Publication January 21st, 1913.)

I. UNIFORM MAGNETIC FIELD.

Let a magnet, the moment of which is μ, vibrate in a given plane in a constant field without friction. The component of the magnetic moment in the direction of the field is $\mu\cos\theta$, when it is deflected through an angle θ, and the mean magnetic moment M in this direction over a period of time T is

$$\frac{1}{T}\int_0^T \mu\cos\theta dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The equation of motion is

$$\frac{d^2\theta}{dt^2} + K\sin\theta = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

hence

$$\frac{d\theta}{dt} = 2K^{\frac{1}{2}}\left(\sin^2\frac{\alpha}{2} - \sin^2\frac{\theta}{2}\right)^{\frac{1}{2}}$$

α being the extreme deflection.

Substituting for T and dt in (1) and inserting a constant c we have the equation

$$cM = \left\{\int_0^\alpha \frac{d\theta}{2K^{\frac{1}{2}}\left(\sin^2\frac{\alpha}{2} - \sin^2\frac{\theta}{2}\right)^{\frac{1}{2}}}\right\}^{-1} \int_0^\alpha \cos\theta \frac{d\theta}{2K^{\frac{1}{2}}\left(\sin^2\frac{\alpha}{2} - \sin^2\frac{\theta}{2}\right)^{\frac{1}{2}}}$$

Putting

$$\sin\frac{\theta}{2} = \sin\frac{\alpha}{2}\sin\phi$$

we get

$$cM = \frac{\int_0^{\pi} \frac{\cos\theta}{\cos\frac{\theta}{2}}\,d\phi}{\int_0^{\pi} \frac{d\phi}{\cos\frac{\theta}{2}}} = \frac{\int_0^{\frac{\pi}{2}} 2\cos\frac{\theta}{2}\,d\phi}{\int_0^{\frac{\pi}{2}} \frac{d\phi}{\cos\frac{\theta}{2}}} - 1$$

$$= \frac{\int_0^{\frac{\pi}{2}} 2\Delta\,d\phi}{\int_0^{\frac{\pi}{2}} \frac{d\phi}{\Delta}} \qquad \text{where } \Delta = \left(1 - \sin^2\frac{\alpha}{2}\sin^2\phi\right)^{\frac{1}{2}}$$

$$= \frac{2E'}{F'} \qquad \qquad (3)$$

E' and F' being definite elliptic integrals of the second and first class respectively.

Assigning to α a series of values from 0 to 180° the relative mean values of the magnetic moment may be obtained. They are given in column (2) in the table underneath the maximum being written 100. From this table it is seen that when the magnet is vibrating to a little more than 130° on either side of its position of rest its mean magnetic moment would virtually vanish. As the oscillations increase beyond 130° on either side of its position of rest the magnet would exhibit a moment in the contrary sense and would apparently behave like a *diamagnetic* substance. Thus ferromagnetic properties might change to apparent diamagnetic properties solely in virtue of the motion of the magnet.

To calculate the mean kinetic energy (e) of this motion we have

$$e = \frac{1}{T}\int_0^T \tfrac{1}{2} I\omega^2 dt = \frac{I}{2T}\int_0^T \left(\frac{d\theta}{dt}\right)^2 dt \quad . \quad . \quad . \quad (4)$$

I being the moment of inertia of the magnet and ω its angular velocity. Also, as before,

$$\frac{d^2\theta}{dt^2} + K \sin\theta = 0$$

and therefore

$$\frac{d\theta}{dt} = 2K^{\frac{1}{2}}\left(\sin^2\frac{\alpha}{2} - \sin^2\frac{\theta}{2}\right)^{\frac{1}{2}}.$$

On substituting in equation (4) we get

$$e = 2KI\left\{\int_0^\alpha \left(\sin^2\frac{\alpha}{2} - \sin^2\frac{\theta}{2}\right)^{-\frac{1}{2}} d\theta\right\}^{-1}\int_0^\alpha \left(\sin^2\frac{\alpha}{2} - \sin^2\frac{\theta}{2}\right)^{\frac{1}{2}} d\theta.$$

Putting $\qquad \sin\frac{\theta}{2} = \sin\frac{\alpha}{2}\sin\phi$

we have

$$e = \tfrac{1}{2}KI\left\{\int_0^{\frac{\pi}{2}} \frac{d\phi}{\cos\frac{\theta}{2}}\right\}^{-1}\int_0^{\frac{\pi}{2}} \frac{\cos\theta - \cos\alpha}{\cos\frac{\theta}{2}} d\phi$$

$$= \tfrac{1}{2}KI\left\{\frac{\displaystyle\int_0^{\frac{\pi}{2}} \frac{\cos\theta}{\cos\frac{\theta}{2}} d\phi}{\displaystyle\int_0^{\frac{\pi}{2}} \frac{d\phi}{\cos\frac{\theta}{2}}} - \cos\alpha\right\}$$

$$= \tfrac{1}{2}KI\left\{\frac{\displaystyle\int_0^{\frac{\pi}{2}} 2\Delta d\phi}{\displaystyle\int_0^{\frac{\pi}{2}} \frac{d\phi}{\Delta}} - 1 - \cos\alpha\right\}.$$

$$= \tfrac{1}{2}KI\left\{\frac{2E}{F} - 1 - \cos\alpha\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Assigning to α a series of values from 0° to 180° the relative mean values of the energy may be computed, as in column (3) of the subjoined table, where the maximum is put equal to 100.

Comparing the numbers in columns (2) and (3) it is seen that the mean magnetic moment decreases more and more as the mean energy increases and that the mean magnetic moment vanishes a little before the energy of vibration has reached its maximum. At the maximum the magnetic moment has a mean value in the opposite sense to that of the controlling field and exhibits the diamagnetic character.

When the vibrations pass into rotations the diamagnetic character will still be in evidence.

II. FIELD DUE TO AN OSCILLATING MAGNET.

Suppose the field controlling the magnet is not constant but is set up by a similar neighbouring vibrating magnet. If the two magnets vibrate freely in the same period and in such a way that the controlling field is proportional to cos θ, where θ is the common angle of deflection from the axis of alignment, then the equation of motion is,

$$\frac{d^2\theta}{dt^2} + K \sin\theta \cos\theta = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and

$$\frac{d\theta}{dt} = K^{\frac{1}{2}}(\sin^2\alpha - \sin^2\theta)^{\frac{1}{2}}$$

α being the extreme angle.

Hence substituting for T and dt in equation (1), and inserting a constant c' we have

$$c'M' = \left\{\int_0^\alpha \frac{d\theta}{(\sin^2\alpha - \sin^2\theta)^{\frac{1}{2}}}\right\}^{-1} \int_0^\alpha (\sin^2\alpha - \sin^2\theta)^{-\frac{1}{2}} \cos\theta\, d\theta\,,$$

Putting $\sin\theta = \sin\alpha \sin\phi$ we have

$$c'M' = \frac{\int_0^{\frac{\pi}{2}} \frac{\cos\theta\, d\theta}{\sin\alpha\cos\phi}}{\int_0^{\frac{\pi}{2}} \frac{d\theta}{\sin\alpha\cos\phi}} = \frac{\int_0^{\frac{\pi}{2}} d\phi}{\int_0^{\frac{\pi}{2}} \frac{d\phi}{\Delta}}$$

where $\Delta = (1 - \sin^2\alpha \sin^2\phi)^{\frac{1}{2}}$ and this may be put,

$$c''M = \frac{1}{F'} \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

For the energy (e') we substitute the value of $\frac{d\theta}{dt}$, derived from equation (6), in equation (4) and obtain—

$$e' = \frac{IK'}{2} \frac{\int_0^{\alpha} (\sin^2\alpha - \sin^2\theta)^{\frac{1}{2}} d\theta}{\int_0^{\alpha} (\sin^2\alpha - \sin^2\theta)^{-\frac{1}{2}} d\theta}$$

Putting $\sin\theta = \sin\alpha \sin\phi$ we have

$$e' = \frac{IK'}{2} \sin^2\alpha \frac{\int_0^{\frac{\pi}{2}} \frac{\cos^2\phi}{\cos\phi} d\theta}{\int_0^{\frac{\pi}{2}} \frac{d\theta}{\cos\phi}}$$

$$= \frac{IK'}{2} \frac{\int_0^{\frac{\pi}{2}} \frac{\sin^2\alpha \cos^2\phi\, d\phi}{\Delta}}{\int_0^{\frac{\pi}{2}} \frac{d\phi}{\Delta}}$$

$$= \frac{IK'}{2} \frac{\sin^2\alpha F' - \int_0^{\frac{\pi}{2}} \frac{1 - \Delta^2}{\Delta} d\phi}{F'}$$

$$= \frac{IK'}{2} \left\{ \frac{E'}{F'} - \cos^2\alpha \right\} \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

Assigning to α a series of values from 0° to 90° the relative mean values of the magnetic moment and energy may be calculated. In the subjoined table, columns (3) and (4), the maximum in each case has been put equal to 100.

The principal difference between this and the former set of numbers is the absence here of negative values and consequently of *diamagnetic* properties. The magnetic moment declines continuously, but the energy reaches a maximum at an angle α approximately equal to 70°. At 90 the magnets are in a position of instability. If such a pair of magnets is in rotation the magnetic moment vanishes.

The behaviour in the simple case here considered of two magnets vibrating freely presents some features which are found in the behaviour of a ferromagnetic substance when its residual magnetism is subjected to rise of temperature. There is in such a substance a decline of magnetic moment which at lower temperatures is slow and at higher temperatures swift, and at or near the critical temperature, where the loss of magnetism is very rapid, the phenomenon of recalescence is found, which shows that the rate at which energy is being absorbed on heating and emitted on cooling is extremely large.

Sir J. A. Ewing conjectures that recalescence may be due in part to the subsidence of rotations of molecular magnets into vibrations of narrow range, and the table shows that the change of energy is very large immediately below 90°, when rotations pass into vibrations.

Single Magnet.			Pair of Magnets.		
a	Mean Magnetic Moment.	Mean Energy.	a	Mean Magnetic Moment.	Mean Energy.
0°	100·0	0·0	0°	100·0	0·0
30°	93·3	10·0	30°	93·1	36·6
45°	85·1	21·5	45	84·7	69·3
60	74·1	36·2	60	72·8	94·5
75	60·8	52·4	65°	68·0	98·8
90	45·7	68·6	70	62·7	100·0
120	12·3	93·4	71	61·6	99·8
130°	0·8	97·7	75°	56·8	97·6
140°	− 10·7	100·0	89	28·9	55·7
150	− 22·2	96·5	90°	0·0	0·0
178	− 63·2	55·1			
180°	− 100·0	0·0			

V. The Specification of the elements of Stress. Part II. A Simplification of the specifications given in Part I. (Vol. lvi., No. X.).

By R. F. GWYTHER, M.A.

(*Read January 7th, 1913. Received for publication, January 20th, 1913.*)

In the first part of this paper* I gave the general solution of the equations

$$\frac{dP}{dx}+\frac{dU}{dy}+\frac{dT}{dz}=0,$$

$$\frac{dU}{dx}+\frac{dQ}{dy}+\frac{dS}{dz}=0,$$

$$\frac{dT}{dx}+\frac{dS}{dy}+\frac{dR}{dz}=0,$$

in the form

$$P=\frac{d^2\Theta_3}{dy^2}-2\frac{d^2\Psi_1}{dydz}+\frac{d^2\Theta_2}{dz^2},$$

$$Q=\frac{d^2\Theta_1}{dz^2}-2\frac{d^2\Psi_2}{dxdz}+\frac{d^2\Theta_3}{dx^2},$$

$$R=\frac{d^2\Theta_2}{dx^2}-2\frac{d^2\Psi_3}{dxdy}+\frac{d^2\Theta_1}{dy^2},$$

$$S=-\frac{d^2\Psi_1}{dx^2}+\frac{d^2\Psi_2}{dxdy}+\frac{d^2\Psi_3}{dxdz}-\frac{d^2\Theta_1}{dydz},$$

$$T=\frac{d^2\Psi_1}{dxdy}-\frac{d^2\Psi_2}{dy^2}+\frac{d^2\Psi_3}{dydz}-\frac{d^2\Theta_2}{dxdz},$$

$$U=\frac{d^2\Psi_1}{dxdz}+\frac{d^2\Psi_2}{dydz}-\frac{d^2\Psi_3}{dz^2}-\frac{d^2\Theta_3}{dzdy}.$$

I also noted the similarity in form of the right hand side of these equations to certain equations of condition

* *Manchester Memoirs*, lvi. (1912), X., equations (5).

which appear in the theory of elastic strains, from which it follows that, if

$$\Theta_1 = \frac{du}{dx}, \qquad \Theta_2 = \frac{dv}{dy}, \qquad \Theta_3 = \frac{dw}{dz},$$

and

$$2\Psi_1 = \frac{dw}{dy} + \frac{dv}{dz}, \qquad 2\Psi_2 = \frac{du}{dz} + \frac{dw}{dx}, \qquad 2\Psi_3 = \frac{dv}{dx} + \frac{du}{dy},$$

the expressions given for P, Q, R . . . will vanish identically.

I however omitted to notice the great simplification which this latter fact allows us to make in the form of the solution of the equations.

For it appears that, whatever forms the functions Θ_1, Θ_2, Θ_3, may have, we may equate them to

$$\frac{du}{dx}, \ \frac{dv}{dy}, \ \text{and} \ \frac{dw}{dz}$$

respectively, and then find some allied functions $\Psi'_1, \Psi'_2, \Psi'_3$, such that

$$2\Psi_1' = \frac{dw}{dy} + \frac{dv}{dz}, \ \text{etc,}$$

and therefore such that

$$0 = \frac{d^2\Theta_3}{dy^2} - 2\frac{d^2\Psi_1'}{dydz} + \frac{d^2\Theta_2}{dz^2}, \ \text{etc.}$$

On subtracting these six equations from the corresponding expressions for P, Q, R, etc., we shall eliminate the functions Θ_1, Θ_2, Θ_3, and alter the values of the corresponding functions Ψ_1, Ψ_2, Ψ_3.

But as all the functions are arbitrary functions, this amounts to the statement that if we equate each of the functions Θ_1, Θ_2, Θ_3, in the values of P, Q, R, to zero, the remaining terms in Ψ_1, Ψ_2 and Ψ_3 alone still constitute a general solution.

By similar reasoning, we may omit the terms in Ψ_1, Ψ_2, and Ψ_3, and the remaining terms in Θ_1, Θ_2, and Θ_3 alone will constitute the general solution.

In a foot note on page 5 of the paper referred to, I stated that my solution differed to this extent from a solution previously given by Sir G. B. Airy, by the introduction of the Ψ- functions. I must now withdraw the implication that Airy's solution is not as general as that which I then proposed.

We may, however, proceed further with the alteration in form of the solutions. We may select any three of the six functions to equate to three elements of strain, provided that we are able to deduce values of the other three functions from the three elements of strain selected, and then we may omit as before the three selected functions from the expressions for P, Q, R, S, T, U.

It seems that the only components of strain to be excluded are such combinations as

$$\frac{du}{dx}, \frac{dv}{dy}, \frac{dv}{dx}+\frac{du}{dy}$$

which do not introduce w, and will not serve for the purpose required.

The sets of functions therefore which may not be omitted from the values of P,Q,R are Θ_2, Θ_3, and Ψ_1; Θ_3, Θ_1, and Ψ_2; Θ_1, Θ_2, and Ψ_3; conditions that are otherwise seen to be necessary.

The results arrived at apply also to the cases of solution in cylindrical- and spherical-polar co-ordinates, and as the expressions in these cases are longer, the convenience of effecting a reduction in the number of functions will be the greater.

The question of the generality of the proposed solution naturally arises, and is not without mathematical interest. It appears that the solution is quite general although the six arbitrary functions can, from the form in which they appear, be so grouped as to be reduced to three.

A justification may be founded on the formal solution of the more general statical equations

$$\frac{dp_{xx}}{dx} + \frac{dp_{yx}}{dy} + \frac{dp_{zx}}{dz} = 0$$

with two similar equations.

From these we obtain in the most general case

$$p_{xx} = \frac{d\psi_x}{dy} - \frac{d\phi_x}{dz},$$

$$p_{yx} = -\frac{d\psi_x}{dx} + \frac{d\theta_x}{dz},$$

$$p_{zx} = \frac{d\phi_x}{dx} - \frac{d\theta_x}{dy};$$

with two other sets of three equations each.

Now making $p_{yx} = p_{xy} = U$, etc., we obtain the equations of which the general solution is sought.

Accordingly we get

$$\frac{d\psi_x}{dx} + \frac{d\psi_y}{dy} - \frac{d}{dz}(\theta_x + \phi_y) = 0$$

$$-\frac{d}{dx}(\psi_y + \psi_z) + \frac{d\theta_y}{dy} + \frac{d\theta}{dz} = 0$$

$$\frac{d\phi_x}{dx} - \frac{d}{dy}(\theta_x + \psi_z) + \frac{d\phi_z}{dz} = 0.$$

Without going through the steps to obtain the consequent values of p_{xx} . . . ., S, T, and U, it is clear that the values will depend on the second differential coefficients of some arbitrary functions, and, if this is the case, the values must be those which have already been found, and accordingly the result must be general.

The fact that by the equality of p_{yx} and p_{xy}, etc., the number of arbitrary functions in the solution of the equations can be reduced to *three* is, I think, established, but on the physical interpretation of the reduction I have not formed a definite opinion.

VI. A Note on Black Pottery from the Gold Coast and Ashanti.

By William Burton, M.A., F.C.S.

(Received and read April 8th, 1913.)

Antiquarians and ethnographers have often drawn attention to the unglazed forms of primitive pottery which are gray or black throughout their substance.

The pre-dynastic races of the Nile Valley, the earliest races in Italy and in many other European countries evidently produced large quantities of pottery of this kind, and the so-called "Buchero Nero" of Northern Italy and elsewhere has acquired great significance in the various attempts that have been made to classify the progressive stages of culture of these races.

It is natural that, owing to this importance of ancient black pottery, many attempts have been made to explain how the pottery was blackened, and it has generally been assumed, where the blackening was thorough and intense, that the ware was fired in some kind of a smother kiln in which, towards the finish of the firing, the vents of the kiln were wholly or partially blocked so as to retain the carbonaceous matter in the kiln, for, of course, it has long been known that this black ware owes its colour purely to the presence of finely divided carbon or some carbonaceous substance through the ware.

Recently I received from the Liverpool Museum some very interesting information, together with some specimens which Mr. Ridyard had kindly forwarded to them, showing how this process of making black pottery is at the present day conducted in the regions bordering on the Gold Coast of West Africa.

The clay used for the purpose, which is an ordinary superficial red clay identical with many red burning clays found in England and elsewhere, and of which I exhibit a sample, is prepared with water in the usual way and allowed to stand in a mass for about two days. Then the women, who are the potters as is usual with all primitive peoples, shape the vessels by hand on a piece of board from the fairly stiff plastic clay, being apparently unacquainted with even the simplest form of potter's wheel. This, of course, is exactly what took place in the making of pottery in the earliest times, for both gray, red and black pottery vessels of excellent shape and of a high degree of finish are well known long before the introduction of the potter's wheel.

When the vessels are made they are dried slowly until they are in a condition which a modern potter would describe as "leather hard." They are then polished with small flattened pebbles such as can be picked up on many sea-beaches, and, as these primitive peoples have a great fondness for the simpler forms of geometrical ornament, the edges of the pebbles are used to incise patterns into the clay, as in the examples shown. A good plastic clay can be polished to a very fine surface by this method.

After making and polishing, the vessels are allowed to stand in the sun for about three days to thoroughly dry and harden them before firing, and in tropical and subtropical countries such sun-dried vessels are often used for the storage of dried grain and other food products, and, by pitching them inside, they can be used for holding liquids.

After drying in the sun the vessels are placed in a conical fire made of sticks placed on end in a bed of dried grass, a number of pieces being fired together. They are fired for forty minutes, and after being removed from the fire in this condition and allowed to cool they are, of course, found to be burnt red. The pots that are to remain red are taken from this fire, cooled, and then coated with a thin coating of red-raddle and again polished with the stone polishers. I have here two fragments of the mineral that is used for this purpose, which is stated to be very difficult to get as it comes from the interior of Ashanti.

To make the black pottery the following ingenious method is adopted:—

The pots are taken from the fire after forty minutes' burning, while they are red hot, and are plunged into a bed of leaves, where they are constantly moved about for twenty minutes. The leaves, of course, become carbonised, and a dense smoke is given off in the process. Care is taken to keep the leaves from firing by damping them if they are too dry. The finely divided carbon or tarry matters produced in this way penetrates into the heated clayware and leaves a deposit of carbon throughout it.

When the vessels are finally taken from the bed of leaves they can be again polished, if necessary, by the use of the stones, and the pottery is complete.

All the operations appear to be conducted under a kind of thatched shed, or are protected from the wind by the use of matting.

The authorities of the Liverpool Museum were kind enough to send not only specimens of the clay and of the materials used but also a sack of the leaves. They requested that I would repeat the process as described both with some of the red clay from Africa and with our own local red clay. I was also asked if there was any special merit either in the clay or in the leaves themselves.

I sent some specimens of the leaves to our President, who has kindly determined them for me, as he explains in a note appended to this paper. It is obvious, not only that any kind of clay could be blackened in this way by the use of these leaves, but that any kind of leaves or dry grass would act in precisely the same way.

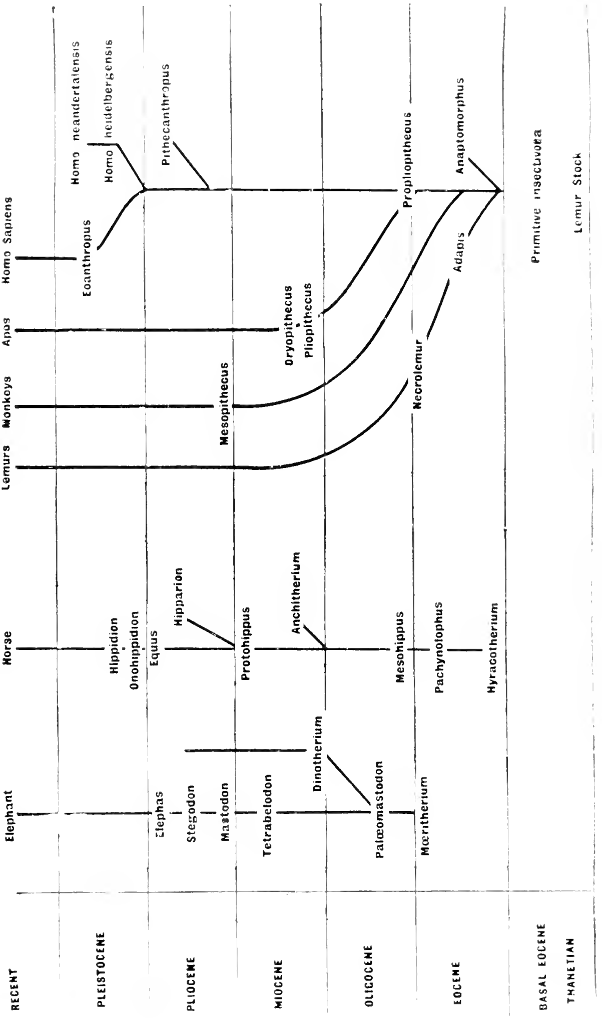
I have here some specimens made at Pilkingtons' Works at Clifton Junction from the African clay and from our own clay, both in the red and the black state, and what is true of the one is, of course, true of the other. Some were blackened with the leaves, others with damp hay or damp sawdust, and they are identical in result.

I am glad to have the opportunity of reading this note to our Society because the method used is undoubtedly one that was well within the reach of primitive people at the most remote periods, when pottery was first made, and any one theorising to-day as to the methods used by ancient peoples ought to be very careful that the methods attributed to them were well within the compass of their knowledge and of the materials at their disposal.

NOTE.

The leaves referred to by Mr. Burton in his note on Black Pottery from the Gold Coast and Ashanti and submitted to me for identification were those of the Cashew-nut (*Anacardium edule*), a West Indian tree now grown extensively all over the tropics. The veins of the leaf are accompanied by very characteristic resin canals. The mesocarp of the fruit contains a volatile oil, which blackens on exposure to the air. It is, however, obvious from Mr. Burton's experiments that it is not any special substance contained in the leaves of the Cashew-nut which turns the pottery black, but that this power is common to every kind of vegetable tissue.

F. E. W.



RECENT
PLEISTOCENE
PLIOCENE
MIOCENE
OLIGOCENE
EOCENE
BASAL EOCENE
THANETIAN
Homo Sapiens
Homo neandertalensis
Homo heidelbergensis
Eoanthropus
Pithecanthropus
Propliopitheous
Anaptomorphus
Primitive Insectivora
Lemur Stock
Apes
Oryopithecus
Pliopithecus
Monkeys
Mesopithecus
Lemurs
Necrolemur
Adapis
Horse
Hippidion
Onohippidion
Equus
Hipparion
Protohippus
Anchitherium
Mesohippus
Pachynolophus
Hyracotherium
Elephant
Elephas
Stegodon
Mastodon
Tetrabelodon
Dinotherium
Palæomastodon
Mœritherium

Manchester Memoirs, Vol. lvii. (1913), *No.* 7.

VII. A Criticism of some Modern Tendencies in Prehistoric Anthropology.

By W. H. SUTCLIFFE, F.G.S.

"FOREWORD."

Although the broad lines of this paper are entirely my own, and I claim the whole of the responsibility in connection with it, I wish to acknowledge my indebtedness to Mr. D. M. S. Watson for much assistance in working out the details.

June 24th, 1913.

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VII. A Criticism of some Modern Tendencies in Prehistoric Anthropology.

By W. H. Sutcliffe, F.G.S.

(*Received and Read March 18th, 1913.*)

Introduction.

Within the last few years there has been a great revival of interest in England in the study of Primitive man which has resulted, not only in much valuable work, but also in the diffusion and general acceptance of ideas based on the most slender and insecure foundations.

It is the purpose of this paper to examine some of this evidence, in so far as it lends support to a high antiquity of man in Western Europe.

The human remains found in America, for which a great age has been claimed, particularly Ameghino's discoveries, have been so completely examined by Hřdlicka, who found that such of them as are human are the remains of recent Indians, that it is unnecessary to pay any attention to them.

Section I.

Before considering the evidence of man's antiquity, it is necessary to clear the ground by understanding the exact point at issue.

It is certain that man has been gradually evolved from some early Eocene stock, and it is probable that his ancestry has followed a different but parallel line to that of the higher anthropoids for a very long period, perhaps since the Oligocene. Hence, just as we can loosely speak of horses being an element in the middle Eocene fauna of Europe, so we can say that man also occurred at that time. To do so, however, is to beg the question at issue. Were it not for the wonderfully complete series of horse ancestors now known, it would be impossible to recognise the Eocene and Oligocene horses (*Pachynolophus*), etc., as in any way related to those now living, and in the same way we should not recognise our ancestors of that period.

For the purpose of this work "*Man*" is taken as including only members of the genus *Homo*, or rather of the family Hominidæ bearing the same relation to the family Simiidæ that the sub-family Equinæ (*Onohippidion*, *Hippidion*, and *Equus*) does to the family Equidæ (*Hyracotherium*, *Anchitherium*, *Hipparion*). This family (Hominidæ) may be taken to include *Pithecanthropus*, *Eoanthropus* and *Homo*.

For the general discussion it may also be taken to include all makers of implements which show evidence of design.

The European evidence of man's high (prepleistocene) existence is of two kinds: (*a*) Direct through the discovery of tools in early gravels, and (*b*) indirect, depending on theoretical conclusions.

(*a*) The direct evidence depends mainly on the chipped flints, commonly called Eoliths. These flints (and cherts) have been found in many places and in deposits of all ages, and have a general resemblance; they are rough, natural pieces of flint which are chipped round the edges. Examples which call for special mention are the series from the plateau gravels of Kent, first collected by Mr. Benjamin Harrison and brought to scientific notice by the late Sir Joseph Prestwich.

The cherts from the middle Miocene of Aurillac are also of importance, and the flints from the basement bed of the Red Crag found by Mr. J. Reid Moir and described by Sir E. Ray Lankester demand special treatment.

(*b*) The indirect evidence depends on the work of Professor A. Keith and is of the following character :—

Two remains of man, the authenticity of which is above question, have been found in early pleistocene gravels : *Homo heidelbergensis,* represented by a lower jaw, from Mauer, near Heidelberg, found in association with a typical early pleistocene fauna (*Elephas antiquus, Rhinoceros etruscus* and *Equus stenonis*), and *Eoanthropus,* found in a high level river gravel at Piltdown, Sussex, associated with a mixed fauna, containing derived Pliocene types. This find consists of a fragmentary skull and lower jaw, undoubtedly belonging to the same individual. The fact of this association shows conclusively that the human remains are *not* derived, for if they had been it is inconceivable that the two separate parts should have been buried in such close association. They are therefore of the same age as the deposition of the gravel, which is fixed by the occurrence of good implements as being early Pleistocene.

The series of implements is probably too small to allow of definite dating, but Dr. A. Smith Woodward and Mr. Charles Dawson, their discoverers, regard them as Chellean ; they are, in any case, River Drift.

These two types are exceedingly primitive, and the later Mousterian type, *Homo neandertalensis,* could on existing evidence be derived from either of them.

Two skeletons, the Galley Hill and the Ipswich man, which have recently been examined by Professor A. Keith, are regarded by some authorities as being of approximately the same age—Chellean or Early Acheulean. They represent essentially modern types of men, utterly different from *Eoanthropus* and *Homo heidelbergensis,* and the conclusion is that man must have already been in existence for a very long period to allow of the development of such divergent types.

This conclusion is logical, but depends entirely on the age of the two skeletons in question, and it is proposed to examine in detail the evidence of this age.

Section II.

THE BROAD LINES OF TERTIARY MAMMALIAN EVOLUTION.

In the basal Eocene of Europe and North America occurs a large mammalian fauna, best known from the discoveries of Lemoine in the Thanetian of Cernay, near Rheims, and of the American palæontologists in the Puerco and Torrijon formations of New Mexico. This fauna contains three distinct elements:—

First, a large number of multituberculates, the last survivors of an important Mesozoic group which practically died out at the end of this division.

Second, the main mass of the fauna consists of very primitive placental mammals, including the ancestors of many archaic types which, with few exceptions, died out at the end of the Eocene period.

Third, a very small group of animals, which may very doubtfully include the ancestors of such modern groups as the Insectivores and Primates.

There is thus a complete absence of all modern types of animals; the ancestral Artiodactyls, Perissodactyls, and Rodents, if they occur, are in so extremely generalised a form that they cannot be recognised. All the Thanetian animals are of small size, none bigger than a sheep.

The first certain Primates occur in the Wasatch (Lower Eocene) of North America in the genus *Anaptomorphus*, a small, very primitive *Tarsius-like* form, with tritubercular molar teeth. The same, and some other genera, occur in the top of the lower Eocene of America, in the Wind River formation, where they attain to a rather larger size.

In the European Upper Eocene we get typical Primates. *Adapis* and *Necrolemur* are lemur-like animals, the largest about the size of a fox terrier.

The associated mammals include many groups which became extinct during the Oligocene and Miocene (Lophiodonts, Palæotheres, Dichobunids, Anthracotheres, Ancodonts, Xiphodonts, Dichodonts, Anoplotheres, and Hyænodons, together with primitive horses, pigs, lemurs, squirrels, insectivores, primates, and carnivores).

No animals which have any claim to be considered the actual ancestors of the ruminants are known.

The first anthropoids in the widest sense of that term have been found in the Oligocene of the Fayûm, Egypt. They represent three distinct genera, one of which has every appearance of being an ancestral higher ape or man. This animal, *Propliopithecus*, which has been described by Schlösser, is of very small size (about that of a new-born human infant) and is solely represented by one ramus of a lower jaw in the Stuttgart Museum.

The associated fauna includes extremely primitive members of the Proboscidea before that group had acquired its characteristic dentition and trunk.

There are also representatives of many families or orders of ungulates which have since died out; and at the same period, in Europe and North America, were living amongst innumerable types whose lines subsequently became extinct the very primitive ancestors of the Horses with three well-developed toes; of the deer, hornless, of very small size, with very primitive limbs; and of the camels and pigs.

In the succeeding Miocene of France we find an ape *Pliopithecus* having affinities with the Gibbons, with a fauna in which true dogs and cats had not appeared; where the deer had very simple antlers, like the existing muntjac, the horse still had three well-developed functional toes, and the Proboscideans included the primitive Mastodon, *Tetrabelodon angustidens* and *Dinotherium.*

In the Pliocene, living types of animals make their first appearance in force; the deer, oxen, antelopes, horses, rhinoceroses, hippopotami, cats, dogs, and bears in, at any rate, the later part of this period, agreeing fairly closely with those now living.

Section III.

EOLITHS AND ROSTRO-CARINATES.

The preceding sketchy account of Tertiary mammalian life will show the complete impossibility of a tool manufacturing ape or man living in the lower Eocene at a time when practically no modern types were even foreshadowed.

In discussing the evidence of tools as to man's antiquity, two features have to be considered. Firstly, the evidence as to the age—that is real occurrence in the terraine in which they are said to occur and the actual age of that deposit; and secondly, the evidence that they are implements. There is a tendency amongst those who work much with Eoliths to argue in a circle. They are liable to say that because a stone fits the hand and might have been used for some purpose, it is therefore an implement.

I went to see Mr. Benjamin Harrison of Ightham, the original discoverer of the Kentish Eoliths. I was amazed at the manner in which he manipulated his Eoliths. If the so-called implement would not fit the right hand, then it was evidently intended for a left-handed man, and the variety of purposes to which they might, could, would, or should have been put was without limit. Evidence of such a character is of little scientific importance.

Mr. Harrison and his followers constantly adduce in support of their claims some of the very rude implements of the Tasmanians, saying, as is perfectly true, that many of them would never be recognised as implements unless they had actually been obtained under such circumstances that there can be no doubt about the matter. They claim that many of the debatable Eoliths are similar to those used by the Tasmanians, and thus arrive at the conclusion that they must therefore be implements. In this conclusion they are not, in my opinion, justified. The fact that a stone is capable of being used as a tool is not evidence that it actually was so used, and it is the latter feature which is essential.

Every unbiassed person who traces the gradual improvement of flint implements from the rough Chellean types, through the Acheulean, Mousterian, and Aurignacian, to the magnificently worked flints of the Solutrian period, must admit that, preceding the Chellean period, there must have been other stages of even more primitive implements; in the Strépyan implements of M. Rutôt we seem to have such types, made out of flint nodules by a deliberate flaking, so as to bring the borders to a more or less sharp but irregular edge.

From the Strépyan to the top of the Acheulean we can trace the gradual improvement of generations of workmen in the manufacture of an implement by the surface flaking of a flint nodule; from the Mousterian period upwards we see the evolution of an implement made out of a large rough flake; in fact, throughout the whole period of flint working we find a definite evolution, sometimes retrogressive, but always such that any series of one age will, without external evidence, suggest its own date.

The Eolithic flints have been divided into stages by M. Rutôt entirely from the geological evidence of their age, though it is impossible from the internal evidence of a group of Eoliths to give any indication of their terraine. For example, M. Rutôt himself determined the stage of some Eoliths found by L'Abbé Breuil in the Thanetian or basal Eocene as Reutelian, a Lower Pleistocene stage. Eoliths, in fact, show *no* evolution, a fact readily explicable if they are produced by non-living agents, but beyond explanation if they are products of man's handicraft, since human products, as is well shown by Palæolithic implements, invariably show a progressive change in form, usually forward in the direction of greater finish and usefulness, sometimes retrogressive, leading to loss of beauty and degraded types. It is essential for reliable evidence of this character that the worked flints depended on should be capable of being produced by human agencies alone. As most Eoliths are of similar types, it is sufficient to show that those types can be produced by accidental causes, not involving design, to destroy the importance of Eoliths as evidence, although it will still remain probable that some of these flints have actually been used by man. (For example, the occurrence of *Homo heidelbergensis* in beds little if any younger than the sands of St. Prest and the Norfolk Forest bed suggests that it is quite probable that the Eoliths described from these latter beds are actually instruments, although I hold *that in themselves* they give no reliable evidence of the occurrence of man at that period.)

Professor Commont has described a series of Eoliths from the base of the Eocene, many of which, if found in Palæolithic gravels, would be accepted as implements without demur. M. L'Abbé Breuil has described a similar discovery in the Thanetian; and Mr. Worthington G. Smith describes and gives cuts of some of nature's forgeries that he found deep in undisturbed clay with flints which show bulbs of percussion, ripple flaking and chipped edges.

The very brief account of mammalian evolution given above will convince any unprejudiced observer that these Thanetian and early Eocene flints must be of purely natural production; the utter absence of any animals which we can suppose capable of designedly shaping a tool at the period of deposition of the gravel which contains them is a certain fact supported by the whole weight of our palæontological knowledge. Yet some of these flints are extraordinarily like implements, and if found in palæolithic gravels would be accepted without question. Some of them are formed by the secondary flaking of a primary flake, itself with a good bulb of percussion; sometimes the secondary flaking takes the form of long narrow parallel flakes, presenting an extraordinary resemblance to that on some neolithic arrow heads. All types of Eoliths may be matched amongst these wonderful flints, including the characteristic "hollow scrapers" of the Kent Plateau.

Monsieur Marcellin Boule has shown that the same types are produced daily in the washing mill of a cement works at Mantes, characteristic Kent Plateau types being amongst the products.

Mr. Hazzledine Warren showed that carts, passing over a road mended by flints, produce Kent Plateau Eoliths of typical form in considerable numbers.

Finally, to remove the last trace of doubt, if any should still remain, M. L'Abbé Breuil found the flakes removed from his Thanetian implements still so closely associated with the blocks from which they came as to show clearly the exact way in which they were formed by the mutual pressure of the stones in the thin layer in which they occurred in the deposits. This recent observation seems to entirely destroy the evidential value of Eoliths as bearing on the occurrence of man in early rocks, although it does not follow that some Eoliths may not actually be of human manufacture and use; in a sense, the boulder that one uses to drive in a tent peg is an Eolithic tool, although it will not generally show any marks of use.

The chipped flints found by Mr. Reid Moir in the stone bed at the bottom of the Red Crag are deserving of close attention and careful treatment. The evidence of their provenance is convincing, and such proof as they present of human workmanship has been very ably put forward by a Committee of the East Anglian Prehistoric Society as well as by Sir E. Ray Lankester. It is eminently desirable that some competent authority, with local knowledge and a mind of sceptical texture, should marshal all the testimony which can be brought together against their human manufacture.

All that is attempted in this paper is to indicate some of the lines on which such an examination might proceed. It must at the outset be clearly realised that the Red Crag may be only so slightly older than the deposits in which the Mauer jaw was found that the *a priori* improbability of the existence of "*Man*" at that period is of slight value, if of any at all, as modern types of animals were already appearing in great force in the middle Pliocene of Europe. The discussion therefore will have to be directly concerned with the flints and the conditions under which they could have been formed.

The scratches with which so many of the flints are marked, although they could, of course, be formed in connection with ice work, are, in my opinion, not of a character which allows of a definite statement that they are of glacial origin. The stones do not, of course, show the sub-angular form characteristic of glaciated pebbles, and the scratches themselves have not that regular parallelism which alone justifies an unqualified demand for ice action. They could in all probability have been formed during movements in a thick bed of gravel, either slowly, by earth creep, or by more rapid land-slides. If this be so, it is not unlikely that the stones were at one time in circumstances where they were exposed to the action of large forces capable of producing fractures of the magnitude required.

With regard to the actual flaking, little can be said; it appears to be at present impossible to distinguish between human and natural flaking merely as such. According to M. Commont and M. L'Abbé Breuil's figures, the chipped flints of Thanetian age, which *must* be of natural origin, show bulbs of percussion and secondary parallel flaking, so that *these* characters vanish as evidence of human workmanship.

Evidence of human workmanship of a chipped flint depends, in fact, on its being of such a shape that it can be handled, and that it can be used for some purpose. It is much strengthened by the occurrence of numerous examples of the same type and still more so by proof of a gradual evolution of form of the implement with time. The flints which Sir E. Ray Lankester has named Rostro-carinate implements occur in considerable number (twenty or more), and can be handled to a certain extent. He offers no satisfactory suggestion as to their use. The idea that the smooth under-surface was used for flattening and smoothing skins seems purely fanciful. The surface is usually more or less concave, and in the more richly flaked examples so roughened by the arêtes of the chips as to be useless for such a purpose. In others it is too small, and in some does not exist. In any case such a view provides no explanation of the lateral flaking It is conceivable that they could have been used as percussion implements, but their form is not all adapted to such use; such a flint as number H, largely flaked all over and with a blunt point, is opposed to such a view.

It is, in fact, very difficult indeed to imagine any use for them. The evidence that they are of human handiwork rests very largely on the occurrence of a number of specimens of similar form. Proof of this particular nature is exposed to a psychological objection. Every geologist who has collected largely from one formation is aware after a time that nodules of certain shapes are much more likely to contain fossils than those of other forms. The result of this is that instinctively and quite sub-consciously he selects such nodules, disregarding, and, in fact, not realising the presence of those of other shapes. Exactly the same thing takes place in the case of flints—the collector is sub-consciously or even consciously on the look-out for certain forms, and will in the end reject stones which do not conform—roughly it may be—to the objects of his search. Perhaps the finest example of this action is to be found in the Kentish "Plateau Eoliths," where the industry of Mr. Harrison has brought together an enormous number of flints grouped into three or four types. The coincidence in shape of many of these is very striking, at least as much so as in the case of the Rostro-carinate flints. Yet no plausible use for them has been suggested.

The vast majority of the Kent Plateau implements are more or less of the nature of *hollow* scrapers, a form which is only of use in rounding sticks, a work probably not largely needed by a very primitive man; there is an absence of forms which are adapted for hunting and preparing skins, the all-essential business of primitive man. Chellean man seems to have got along very well without hollow scrapers.

The work of Mr. Hazzledine Warren, L'Abbé Breuil, etc., has, in my judgment, completely discredited the idea that these are of other than natural origin, and I believe that they have now few supporters of importance, or none. The fact that Kentish Plateau types do not commonly occur in any other locality is probably due to the fact that nowhere else do we find suitable gravels rich in the thin tabular flint from which they are all shaped by nature. The resemblance between Mr. Moir's Rostro-carinate flints is not close; some of them have a sharp anterior ridge; others are flat in front. Some slope down gradually to the point; some again drop suddenly so that the keel is nearly vertical. These differences, although just what one would expect if they were of natural origin, are such as to throw serious doubt on their being tools, for they would imply adaptation to very different uses, and the whole argument, from resemblance between the members of a series, turns on the fact that the members of such a series are designed for one particular use.

Perhaps the strongest evidence in favour of the human origin of the sub-crag flints lies in the suggested traces of an evolution in workmanship of the type shown in examples of mid-glacial age. It must be remembered, however, that these later flints are very few in number, and that the best of them were collected by a quarryman presumably accustomed to Palæolithic flints, hence not unlikely to be a plausible illustration, ruder types having been inevitably overlooked.

This evidence is now discredited by some flints which recently came into my hands. Amongst some implements from Hackney (obtained from the well-known dealer, Mr. F. H. Butler) are four flints which agree very closely with Mr. J. Reid Moir's Rostro-carinate type. They agree exactly in colour, patination, and the character of the flint with typical early Acheulean or Chellean implements from the same locality. Four of them are described and two figured in detail in an appendix to this paper.

Comparison of these descriptions and figures with those given by Sir E. Ray Lankester of sub-crag flints will show the complete resemblance of the two series in shape, size, character of the flaking, and general roughness. Sir E. Ray Lankester has already shown that the same form occurs in the upper Miocene of Aurillac.

It is inconceivable, in view of our present knowledge of early flint industries, that a tool should retain exactly the same form and rudeness over this enormous period. I have little doubt that search would show the same form in many of the flint-bearing gravels.

It would be interesting and instructive to collect *all* the flints from the Red Crag stone bed which show any traces of flaking, without reference to their form, and then compare the series so arrived at with the Rostro-carinate and other types described by Sir E. Ray Lankester.

A review of the whole subject shows it is desirable that more extended investigations should be made into the results of natural flint fracturing; perhaps one of the simplest methods of beginning such an investigation would be to collect chipped flints from some early Tertiary flint gravel from which human artifacts would necessarily be absent.

It is very desirable that the Rostro-carinate flints should be submitted to as many scientific archæologists as possible. Especially it is to be desired that the scientific world should benefit from such results as the further examination and unrivalled experience of Professors Boule, Cartailhac, Breuil, and Commont would be likely to confer upon it.

I have presented the four Rostro-carinate flints to the Manchester Museum, together with a series of associated palæoliths.

Section IV.

Human and Animal Remains.

Some years ago, in the Galley Hill pit, in the 100′ Thames terrace in Kent, a skeleton was found which is believed by some authorities, notably Keith, to be contemporaneous with the early Pleistocene mammalia and the Chellian implements found in the same gravels. This skeleton is of an essentially modern type, and if it be correctly referred to the Chellian is older than *Homo neandertalensis* and of the same age as *Eoanthropus*. This would imply a sufficiently long antiquity of man prior to the Pleistocene to allow for the separation of the two stocks, one leading through *Homo heidelbergensis* to *Homo neandertalensis* and the other to the Galley Hill man.

This conclusion is logical, but depends entirely on the age of the Galley Hill man. Quite recently another skeleton of modern type, for which a high antiquity is claimed, has been found by Mr. Reid Moir at Ipswich. These two finds are the only evidence of the occurrence of a modern type of man at this early date, and it is very desirable to examine their authenticity.

It is to be noticed first of all that they are, with the exception of *Eoanthropus*,* the only human bones which claim to have been found in our undisturbed Pleistocene deposits, *not in caves*, and that they are, or were, when found, both complete skeletons. Many *thousands* of bones of animals comparable in size with man have been found in Pleistocene river deposits in England, and only one mountable skeleton is known, a hippopotamus from Barrington, near Cambridge. Even small associated sets of bones are of extreme rarity; five only have been placed on record in museums: *Bos primigenius*, Ilford (Brit. Mus. Nat. Hist.); *Hippopotamus major*, Barrington (Cambridge Mus.); *Rhinoceros antiquitatis*, Crayford (Manchester Mus.); and the remains to which Sir Antonio Brady's Mammoth skull belonged.

It may be objected that complete skeletons have occurred at Galley Hill and the neighbouring pits. This cannot be denied, but they do not assist the argument, since the relics are undoubtedly those of recent cattle, a specimen of which collected by D. M. S. Watson eight feet below the surface in the Milton Street pit has been deposited in the Manchester Museum. Continental experience in river gravels agrees with our own in that separate bones are common enough, but associated sets of bones unusual. The chances therefore against the only specimen of a type being a skeleton are enormous. The

* And the Bury St. Edmunds fragments.

probability that the next specimen of the same type should also be a complete skeleton is so minute as to be negligible ; yet in face of this extreme improbability we are asked to believe that the first two occurrences of man in British Pleistocene gravels should both be complete or essentially complete skeletons. The mistake made by the finders of these skeletons is in presuming that they are of the same age as the gravels in which they were found. What is the evidence?

The Galley Hill skeleton was not seen *in situ* by any scientific man ; in fact, not seen by any geologist till several years after its discovery. Two witnesses of no geological training say that the beds of gravel above were undisturbed. One of them says, "I was struck by the undisturbed condition of the gravel in which it was embedded." The other declares that it "projected from a matrix of clayey loam and sand." On the evidence of these two witnesses, who contradict one another on a point so fundamental as the character of the surrounding matrix, we are asked to believe that the gravel above was undisturbed, a point always very difficult to establish. As a matter of fact, the bones are of a deep brownish grey colour, supporting the view of the witness who said that they lay in clayey loam and entirely different from what we should expect if they had been preserved in the ordinary red brown gravel.

The Ipswich skeleton was found and very carefully excavated by Mr. Reid Moir, so that the evidence in its favour is more satisfactory than that of the Galley Hill man. It lay apparently in a compact mass at the junction of some sands and overlying boulder clay, embedded partly in each. Does Mr. Reid Moir seriously mean to contend that so fragile a thing as a human skeleton could remain closely articulated and unbroken whilst the glacier which deposited that boulder clay passed over it? Mr. Reid Moir also claims that, because the cranium was filled with a compact mass of clay (giving a perfect brain cast), such infilling could only have been formed by a wet liquid slip derived from a glacier; but surely the action of worms and of rains would fill the skull easily enough, even if it had been buried. The whole evidence depends absolutely on the undisturbed condition of the clay above; and it must be borne in mind it is always difficult to prove the undisturbed condition of any unbedded rock, particularly when the cover is very thin, as it was in this case. In neither the Galley Hill skeleton nor the Ipswich skeleton is there any internal evidence of great antiquity, whilst the evidence that they are not merely burials rests, as it has been shown, on so slight a foundation that it cannot for a moment stand against the inherent improbability of the occurrence of two complete skeletons as the sole representatives of a rare type in British Pleistocene strata.

Moreover, I visited the place at Ipswich where the skeleton was found. I was taken there by the workman who discovered it, and after a very careful examination of all the evidence, I came to the conclusion that it was an intrusive burial in which a grave was dug down through the boulder clay to the mid-glacial sands below.

The actual position of the Ipswich man, as shown in Professor Keith's figures, with the knees up under the chin and the hands crossing them, is absolutely inconsistent with any view except that of burial; even the finest skeletons found fossil are usually slightly scattered and spread about.

CONCLUSION.

From a consideration of what has been stated in the foregoing portion of this paper, the whole evidence carefully weighed points to the undoubted conclusion that the skeletal remains of both the Galley Hill and Ipswich man were individuals of *Homo sapiens* and were simply relics of burials of a comparatively recent date; both are complete skeletons, both resemble the modern type of European, both were found in conditions which make burial not only possible but immensely probable.

The removals of these Galley Hill and Ipswich men from the early Pleistocene to a much later period simplifies considerably our views on the descent of man.

Déchelette states in his Manual of Anthropology: "Moreover, as Professor Boyd Dawkins remarked, 'it would be committing a palæontological anachronism to look for traces of a human being in a horizon of the earth's history so remote as the beginning of the Miocene, anterior to the age of mastodons. The presence of man in the Pliocene epoch before the full development of the animal kingdom, of which our first ancestors were the crown, would not surprise naturalists in any manner if it could be proved. It is not the same for the Miocene period.'" Mons. M. de Lapparent writes: "During the epoch when the flints of Thenay were formed it is certain that the animal population of our planet was very incomplete. Hardly had herbivores commenced to develop; ruminants had not yet any horns; there were no horses properly so called, nor Proboscideans." To ask Palæontologists to accept the artificial character of the Thenay flints contemporaneous with *Anthracotherium*, a pachyderm more ancient than the mastodon, itself the ancestor of elephants, it would be necessary to find them, as G. de Mortillet remarked, among the Anthropoid monkeys, such as *Dryopithecus*, belonging to the Upper or Middle Miocene, but hypothesis so serious would demand to be supported by material evidence of higher character than the shapeless fragments gathered by L'Abbé Bourgois and the followers of his teaching.

NOTE.

Since the greater part of the above paper was written I have seen the excellent paper by Mr. F. N. Haward on "The Chipping of Flint by Natural Agencies" in the Proceedings of the East Anglia Prehistoric Society, which is remarkable for its common-sense standpoint. Our conclusions and methods of attack are similar.

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DESCRIPTION OF PLATES.

Description of four flints similar to those described by Sir E. Ray Lankester as Rostro-carinate implements. All are derived from the High Level gravels of Hackney Down, North London, and are associated with implements of exactly similar material and patination of ordinary Chellian type.

α. HACKNEY GRAVELS. O. 1872. Manchester Museum.

A large flint partly honey-coloured and translucent, the lower surface stained a bright yellow orange, fine lustrous patina.

Ventral surface formed by one large conchoidal fracture with the anterior part removed by another conchoidal surface less stained and patinated and hence a later date. The left edge is removed by a narrow longitudinal flake.

Dorsal surface with a high keel running posteriorly into a patch of original surface.

Right side formed by one large flat fracture surface cut into by a conchoidal fracture starting from the lower edge.

Left side also formed by a single surface, the lower ed being removed by the flake described above.

Beak rounded and formed by five small flakes.

All edges and angles battered.

Flint terminated behind by one fracture surface.

Compare with *Phil. Trans.*, B., vol. 202, p. 303, figs. 12 and 13.

β. HACKNEY GRAVELS. O. 1874. M. M.

A medium-sized ocherous flint.

Ventral surface flat, not formed by any definite fractures, but not original crust.

Dorsal surface vaulted, with a keel anteriorly; left posterior region formed by natural crust, the rest by indefinite fractures.

Right side near the beak formed by one small conchoidal fracture.

Left side formed by two conchoidal fractures and numerous secondary flakes.

All edges and corners much battered.

γ. HACKNEY. O. 1875. M. M.

A small flint of grayish colour, considerable patination and no staining.

Ventral surface flat, formed by the natural surface of the flint, except for a minute chip under the beak.

Dorsal surface with a pronounced median ridge, which is rounded.

Right surface formed by a single fracture, shewing no primary bulb of percussion.

Left surface formed by a single fracture, modified by three subsidiary flakes, with bulbs of percussion at the lower edge.

Beak formed by one small flake on the top side and a small fracture on the dorsal surface.

Point and edges battered.

cf. Phil. Trans., B., vol. 202, p. 302, figs. 10, 11.

δ. HACKNEY. O. 1873. M. M.

A large flint of grayish brown colour and considerable polish.

Ventral surface formed by an irregular natural surface posteriorly, by two smooth flat fracture surfaces anteriorly.

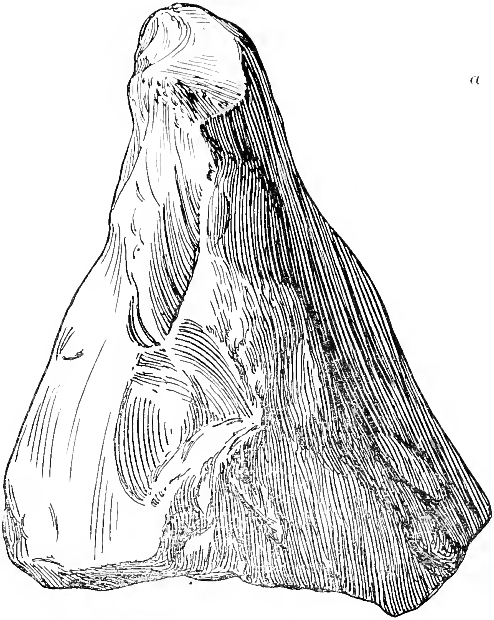
Dorsal surface largely formed by original crust, one large flake in the middle and several posteriorly, a small flake across the beak.

Right surface formed by two large flakes.

Left surface. One large fracture and a small anterior chip forming the beak.

Beak and all other edges rounded and battered.

cf. Phil. Trans., B., vol. 202, p. 308, figs. 20, 21.



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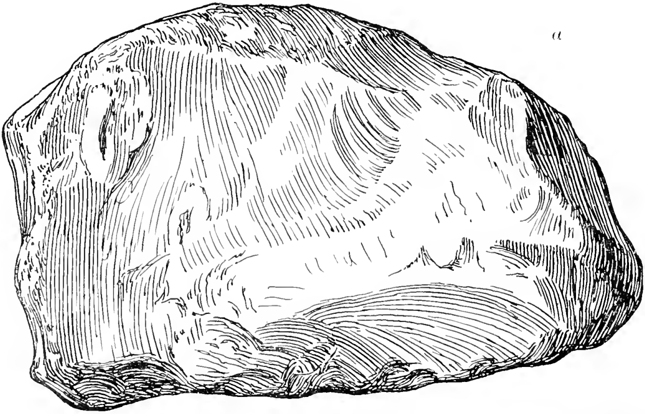

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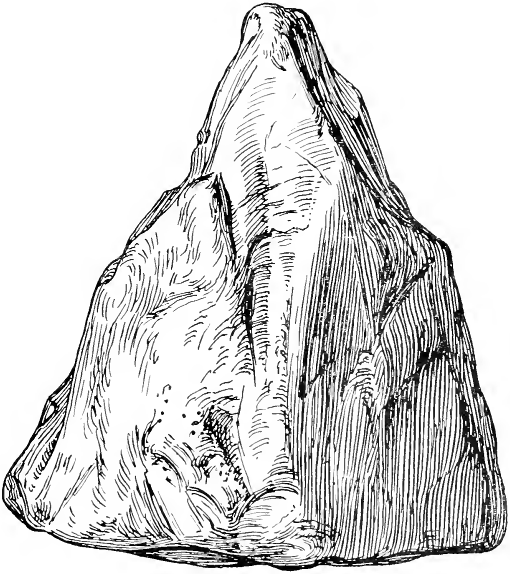

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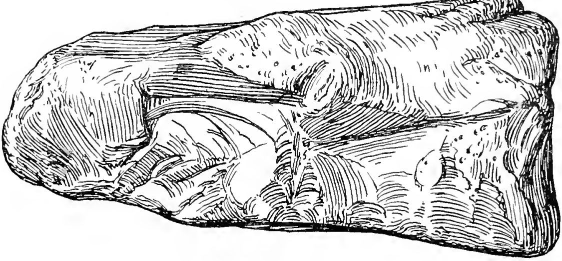

Manchester Memoirs, Vol. lvii. (1913), *No.* 8.

VIII. The Structure and Life-History of *Leptosphæria Lemaneæ* (Cohn).

By William B. Brierley, M.Sc.,
Assistant Lecturer in Economic Botany and Demonstrator in Botany in the University of Manchester.

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History.

Leptosphæria Lemaneæ (Cohn), a Pyrenomycete inhabiting sexual shoots of *Lemanea fluviatilis* was discovered by Wartmann (**17**) in 1854.

In 1856 Montagne (**11**) published his Sylloge Cryptogamarum, but from this the fungus was omitted.

Cohn (**4**) in 1857 briefly described it under the name *Sphæria Lemaneæ*, and stated it to be peculiar to its algal host. He again referred to it two years later.

Beketoff (**2**) mentioned it in 1862, but it was only with the publication of Woronin's (**7**) investigations in 1870 that knowledge of its structure and life-history became known.

In 1887 De Bary (**8**) questioned Woronin's interpretation of the reproductive structures of this fungus.

In the same year Rabenhorst's (**13**) Cryptogamen Flora, Pilze Abt. II., appeared, and in this *Sphæria Lemaneæ* was placed in the genus *Leptosphæria*, following Saccardo (**14**). Lindau (**10**) retained it in this position in Die Natürlichen Pflanzenfamilien of 1897.

In 1901 Lemmermann (**9**) published a compilation of fungi parasitic or saprophytic upon Algæ, but from this *Leptosphæria Lemaneæ* was omitted.

Overton (**12**) in 1906 and Stone (**16**) in 1912 have quoted Woronin in their publications.

The principal features of my investigation of *Leptosphæria Lemaneæ* were completed before I had seen Woronin's account, and with slight additions confirm his results.

HOST PLANT.

Lemanea is an exclusively freshwater genus belonging to the group Nemalionales of the Rhodophyceæ, and grows in tufts attached to submerged rocks in flowing water.

The structure of the sexual shoots, important to an understanding of the morphology of the fungus, is well known (Sirodot (**15**), Atkinson (**1**)), but may be summarised here. The thallus is cylindrical and hollow, and from apex to base runs an axial row of tubular cells, the "Zentralfaden." At more or less regular intervals this is connected by a whorl of four ray cells to longitudinally running generative filaments closely applied to the inner surface of the thallus. Arising from these filaments, and penetrating through the cortex to the periphery, are the procarpia. Each of these, after fertilisation, gives rise to ooblastema filaments, from which are budded off carpospores, which radiate into or almost fill the lumen.

The tissue is somewhat cartilaginous and composed of three zones of cells—a peripheral layer of small, very closely set prismatic cells, containing chromatophores; a middle layer of larger polyhedral cells; a medullary layer of large rounded cells. Mucilage fills the intercellular spaces, surrounds the shoot, and thickly lines or fills the lumen. At more or less regular intervals on the surface of the thallus, and bearing relation to the ray cells, are the antheridial whorls. (See *Text-fig.* 4 for a transverse section of the *Lemanea* shoot.)

METHOD.

Killing and fixing reagents used were weak Flemming's solution, and Chromo-Acetic acid (Schaffner's formula). The material was taken through ten per cent. glycerine to absolute alcohol, cleared in chloroform, and embedded in wax with a melting point of 54°C. Microtome sections were cut varying from 2-8 μ in thickness. The stain found to give best results was Heidenhain's iron hæmatoxylin with bismark brown, light green or erythrosin as a counterstain. Many observations were made upon thickly cut hand sections and macerations of fresh and preserved material.

MYCELIUM.

Woronin describes the mycelium of *Leptosphæria Lemaneæ* as being "strongly developed in the hollow central portion of the thallus, but extending into the parenchymatous tissue where fructification occurs. For the most part the tissue is only affected between the cells. The hyphæ are fairly fine, delicate, hyaline, and possess numerous cross walls. Their branchings are very irregular, and at points of contact fusion not infrequently occurs."

This description is in accord with my observations, but the mycelium perhaps merits a fuller treatment.

The hyphæ within the cylinder of the host run in a generally longitudinal direction and are more or less confined to the thick layer of mucilage lining the lumen. Frequently, however, strands are applied about the axial filament. These hyphæ shew two forms. The one consists of fine, much-branched threads, which by fusion at points of contact and by bridge connections, form an anastomosing cylindrical network; the other of fewer, stouter threads, little branched, and usually pursuing a somewhat straight course for some distance. (*Pl.* I., *Fig.* 1.) The individual cells are regular, colourless and uninucleate. Frequently the contents appear finely granular, and occasionally minute, highly refractive substances are present. Arising as branches from this lumen mycelium, and as secondary hyphæ from the perithecia, is a weft of threads which spreads very extensively in an intercellular manner, principally through the middle and medullary layers of the host tissue. It is from this portion of the mycelium that the fruiting bodies take origin. Branches from the intercellular hyphæ and occasionally directly from the hyphæ of the lumen enter the host cells. The protoplasm of *Lemanea* cells is spread in a thin peripheral layer about a large central vacuole and the fungal haustoria usually directly enter the latter, within which they freely branch. (*Pl. I., Fig.* 2.) Not infrequently the haustoria appear to enter the protoplasm. (*Pl. I., Fig.* 3.)

Lemanea cells are connected by minute pits, the protoplasmic connection being severed by the middle lamella. These pits afford points of entry to the haustoria, but the latter also directly penetrate the cell walls in the absence of pits.

The mycelium of *Leptosphæria Lemaneæ* exhibits hyphal dilations, terminally or in an intercalary position. Not infrequently a dumbbell-shaped structure is formed by dilation of a hypha immediately external and internal to a cell wall. (*Pl. I., Fig.* 4.) These hyphal swellings may or may not be cut off by septa, and usually contain somewhat denser and more refractive contents.

In many cases, as Woronin noted, a vacuole may be present.

REPRODUCTION.

Woronin remarks that "in addition to the perithecia no other reproductive organs have been found in *Sphæria*

Lemaneæ. There are, however, signs of the production of structures which may be undeveloped perithecia or disorganised pycnidia." The genus *Leptosphæria* is stated to be "rich in all forms of conidia" (10).

Pycnidia have been described by Cotton (5) in the marine form *L. chondri*, and they are known in terrestrial species of the genus. In *L. Lemaneæ* I have found neither conidia nor pycnidia, and it is probable that the structures noted by Woronin were immature perithecia as suggested.

A summary of Woronin's account of the development of the perithecium may be given:—"A hypha swells at its tip and the globular termination is cut off by a wall. A dilated hypha from the same mycelium applies itself to this archicarp. The next stage of development was not seen, but from analogy with Erysiphe, Pezizeæ and Ascoboli (6), (7), it was inferred that the club-shaped hypha is cut off by a wall, and that by continued growth and repeated branching the entwining hyphæ finally form a mass of tissue completely enclosing the globular terminal cell or archicarp. The tissue early develops, the walls cells becoming polyhedral by pressure, and assuming a brown coloration. The internal tissue of indistinct small cells forms the so-called perithecium nucleus, which later develops into asci. Paraplyses are absent."

De Bary (8) remarks of this—"incipient sporocarps or archicarps, of doubtful character, and requiring a fresh examination, have been assigned by Woronin to *Sphæria Lemaneæ*."

In macerations it was found, though rarely, that two hyphæ were applied to one another, and that adjoining slightly dilated cells terminal or intercalary, or terminal with intercalary, shewed the appearance of fusion.

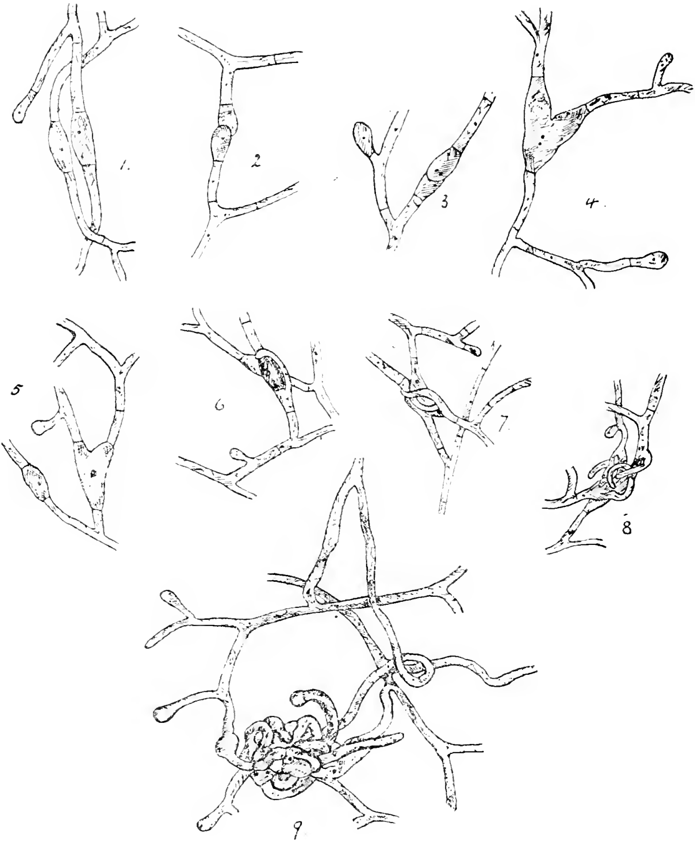

FIG. I.

1—5. Stages in conjugation.

6—9. Growth of enveloping hyphae about the ascogonium.

(Original sketches.)

In such cases one cell was apparently devoid of nucleus, whilst the contiguous cell contained two nuclei or a single nucleus of about twice the normal size. (*Text-fig.* 1, 1—5.)

Microtome sections shewed in intercellular positions hyphal dilations containing two nuclei (*Pl. I., Fig.* 5), and in cases where the adjoining cells could be distinguished there was good reason to think that fusion had occurred. (*Pl. I., Fig.* 6.) Not infrequently such intercellular hyphal swellings contained a single nucleus of about twice the normal size. (*Pl. I., Fig* 7.)

Woronin did not assert a fusion of the structures he described although his *Fig.* 3 would indicate that such had taken place. His archicarp and applied hypha are exactly paralleled by mycelial dilations of purely somatic value, and from my study of this form I find it difficult to believe that the former are of reproductive character. The lower portion of his *Fig.* 5 shews a fusion resembling more nearly those I have seen—fusions which bear marked likeness to those described for *Pleospora herbarum* by Cavara and Mollica (**3**).

From immediately adjoining cells arise fine hyphal branches, which apply themselves closely about the central cell or ascogonium. In macerations this appears as a few closely entwined hyphæ (*Text-fig.* 1., 6—9), whilst in microtome sections a large, more deeply stained uninucleate cell is seen, surrounded by smaller cells. (*Pl. I., Figs.* 8—10.) The enveloping hyphæ increase in size and number, enclosing the ascogonium, until a small spherical lenticular mass is formed.

The fusion nucleus divides (*Pl. I., Fig.* 11), and the ascogonium septates into a number of multinucleate cells (*Pl. I., Fig.* 12), from which arise ascogenous hyphæ.

These branch freely and very irregularly in the perithecial tissues, and the nuclei pass into them in pairs. (*Pl. I., Figs.* 13, 14.)

Walls are laid down, and from apparently any of the binucleate cells thus formed asci may arise.* (*Pl. I., Figs.* 13, 14.)

The binucleate stage of the ascus lasts for a short time only, the nuclei rapidly fusing.

During this development of the asci the enveloping hyphæ have continued to grow, and the necessary space is gained by the pressure apart and degeneration of the host cells in contact with the ascocarp. There is usually early differentiation of the surrounding tissues, the wall layer becoming apparent as consisting of somewhat elongated and flattened polyhedral-shaped peripheral cells, from which intercellular hyphæ and haustoria arise. These cells are especially active in growth, and remain thin-walled until the perithecium is nearly mature. Immediately internal are a few layers of firm, thin-walled tissue, whilst centrally, amidst which the ascogenous elements lie, are very delicate cells. The components of this sterile perithecial tissue are somewhat flattened and polygonal in shape and constantly uninucleate. They do not hold stain, and stand in contrast to the more deeply stained multinucleate ascogenous elements (*Pl. I., Fig.* 12), (*Pl. II., Fig.* 16.)

The asci develop rapidly and soon appear as swollen oblong sac-like bodies, flattened by pressure in their

For a discussion of the interest of such paired nuclei see—
J. H. Faull, "Cytology of *Laboulbenia chætophora* and *L. gyrinidarum.*" *Anns. Bot.*, XXVI. 1912.

* For a different interpretation of this and the similar cases described by Faull ("Development of the Ascus and Spore Formation in *Ascomycetes*," *Proc. Boston Soc. Nat. Hist.*, 32, 1905), see Brown ("The Development of the Ascocarp of *Leotia.*" *Bot. Gaz.*, 50, 1910).

middle regions, and with the sterile cells pressing in between their ends. The surrounding delicate thin-walled tissue subsequently becomes collapsed and devoid of contents, suggesting that it has functioned as nutriment to the developing asci.

Very young fruiting bodies are more or less globular in form, the fertile elements occupying the centre. Later growth is not equally centrifugal, the development being greater toward the host periphery than toward the lumen. (*Pl. II., Figs.* 16—18.) The result is a pear-shaped structure, the tapering mass of tissue occupying the position of the perithecial neck. When the ascocarp has reached its mature size the wall layer becomes pigmented and the cells slightly thickened at their angles. (*Pl. I., Fig.* 15.) This does not extend over the distal surface of the neck or ostiolar pad of tissue, but around it the pigmentation and thickening are present in slightly increased amount. The ostiole or canal is finally formed by the degeneration of this pad of sterile thin-walled tissue.

Woronin described the mature perithecium as follows: "A fully grown normally developed perithecium attains a size of about ·05 mm., and has a rounded barrel or club-shaped form. The entire perithecium is sunken in the *Lemanea* thallus, only the ostiole appearing through the cortical layer of the parenchymatous tissue of the alga. The walls of the perithecium consist at the sides and the apex of one or two layers of polygonal cells, with dark brown coloration. On the base of the perithecium, internal to the brown coat, is a delicate fine-celled parenchymatous tissue, whose elements are arranged in three to five irregular layers. This is the so-called hymenial layer, from which the asci, formed from the perithecium nucleus, arise."

To this concise and accurate description I can add little.

The perithecia occupy the radial thickness of the host tissue, and, although not infrequently contained within a single medullary cell, more usually occupy two or more. (*Text-fig.* 4.) Toward the attenuated ends of the *Lemanea* shoot the cells are smaller, and here the perithecia, apparently of constantly slightly greater size (·06–·07 mm.), occupy part or the whole of the lumen. Two or three perithecia often develop in contiguity, and in such cases the perithecial wall frequently remains undeveloped at the point of contact. The result is a large compound perithecium, opening by the several ostioles. Very rarely instances may be found in which the perithecia shew reversed orientation, that is, the bulbous base is toward the periphery of the shoot. In such cases the ostiole is undeveloped, and the asci which are directed to the lumen do not dehisce, the spores germinating *in situ*. In the so-called hymenial layer of the fruiting body ascogenous hyphæ, usually containing paired nuclei, may be seen. (*Pl. I., Fig.* 15.) These continually develop, so that various stages in ascus growth from binucleate dilated tips to mature asci are to be found within the same perithecium. The younger asci are situated principally at the periphery, and development proceeds in a centripetal direction. The most careful examination of the nuclei failed to reveal the nuclear membrane, and all that could be seen was the prominent nucleolus, usually surrounded by a clear area of protoplasm. Uninucleate asci of different sizes and asci containing spores in various stages of development are numerous, indicating that these stages last for some considerable period of time.

Woronin describes briefly "the division of the nucleus, which is usually in the middle of the sac, into eight, the surrounding of each by an ill-defined protoplasmic area and the subsequent formation of a definite wall, the division of each spore into two, and each daughter cell again into two, resulting in a spore with four cells arranged in a row, and each cell possessing a clearly defined nucleus."

The divisions within the ascus occur simultaneously and rapidly, but the minuteness of the nuclei rendered unsuccessful attempts to observe the mechanism of division.

The mature asci are shortly stalked or sessile club-shaped sacs filled at maturity by the eight fusiform or spindle-shaped perfectly hyaline spores. The spores are described as brownish, and hyaline only when young (**13**). A large number of spores in all stages of development and germination have passed under observation, and in no case has coloration been seen.

Spore Ejection.

The process of dehiscence fully described by Woronin is by successive spore ejection, a method not uncommon in the Pyrenomycetes, and particularly the Sphæriaceæ. "The ascus is double contoured, the two membranes having different osmotic values. The outer tears at its apex, and the contents enclosed by the inner pellicle, which is attached at its base to the outer envelope, immediately expand to two and a half to three times their original size. The inner pellicle swells immediately by absorbtion of water, and the spores along with a little unused protoplasm are successively ejected with some impetus through an apical pore. The elongation brings the tip of the ascus to the ostiole, and so enables the spores of the fungus to be dispersed."

Woronin states that the whole process occurs almost instantaneously, but I have observed that the time elapsing between the first and final stages of ejection may vary from a few seconds to forty-eight hours. In cases where a lengthy period intervened, the spores were

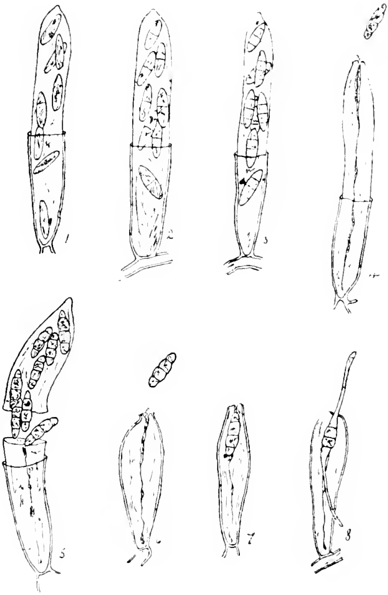

FIG. 2.

1—3. Asci after rupture of outer envelope, containing immature spores in extended inner pellicle.
4. Ascus after final dehiscence.
5. Transverse rupture of ascus.
6. Ascus after simultaneous rupture of inner and outer envelopes.
7 8. Spore germinating in ascus neck.

×750.

frequently immature on rupture of the outer membrane, and their maturation took place within the extended inner pellicle prior to final dehiscence. (*Text-fig.* 2, 1—4.)

Woronin states that sometimes the internal pressure is so great that the inner envelope is split transversely, the tip being thrown off as a small cap (Mützchen oder Fingerhut). (*Text-fig.* 2, 5.) This occurred twice only in my observations.

Asci delicately isolated in a drop of water shew very readily the above types of dehiscence, and also a method not mentioned by Woronin—simultaneous ejection. The internal pressure ruptures the inner and outer envelopes simultaneously at the tip, and the spores, given a slightly rotatory movement by their somewhat spiral arrangement within the ascus, and their fusiform shape, are ejected with great rapidity and some degree of violence. At the same moment the internal membrane swells so quickly that a spore is frequently caught in the ascus neck, there to germinate. (*Text-fig.* 2, 6—8.) This method of dehiscence may be due to differences in tension and strain induced by the mechanical isolation of the asci; but in perithecia, asci may not infrequently be seen in which a spore is germinating in the neck. In a large number of cases the spores are not ejected, but germinate *in situ*; and old perithecia may often be found completely filled by germinating spores.

Spore Germination.

Woronin paid considerable attention to germination studies, infecting healthy filaments in a watch glass, by allowing spores to fall upon them from *Lemanea* containing mature perithecia. Germination occurred in from two to three hours, the hyphæ arising always from the end cells of the spore. The infection was observed, but cultures could not be retained alive for a longer period than two weeks.

Dehisced spores are abundant on *Lemanea* shoots in the vicinity of perithecia, and by careful maceration the manner of infection may readily be seen. For more care-

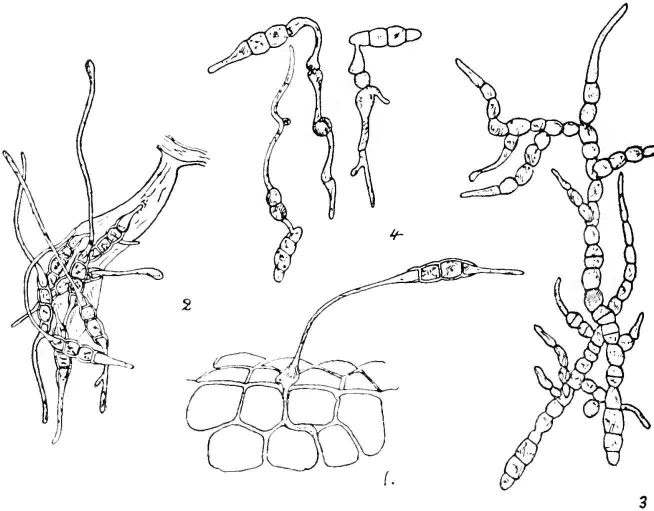

FIG. 3.

1. Infecting hypha shewing appressorium and intercellular mode of entry.
2. Spores germinating in ascus.
3. Spore germination in beerwort.
4. Spore germination in peptone.

× 750.

ful examination, however, infection was brought about by a method similar in principle to that used by Woronin, but giving more exact and controllable conditions. The infecting hypha figured by Woronin is penetrating a cortical cell; whilst all the cases I examined shewed that the infection occurred in an intercellular manner, and a slight dilation or appressorium is present where the germ tube comes into contact with the host. (*Text-fig.* 3, 1.)

Attempts were made by placing cultures under simulated natural conditions to prolong their life, but three weeks was the maximum period for which they could be retained.

A large series of hanging drop cultures were made.

In distilled, sterilised and tap water the spores gave a small amount of long, attenuated hyaline mycelium, with little protoplasmic contents and few walls. The spores became emptied of matter and the walls appeared to become thinner. Hyphal dilations were present in the mycelium, and death ensued after seven to ten days.

In various nutrient media the cultures were more successful. Dehisced spores, mature asci with spores *in situ*, asci with two and one-celled spores, and even asci with undifferentiated contents readily germinated. (*Text-fig.* 3, 2.)

Beerwort cultures gave a curious initial growth of stout, irregular, much-branched hyphæ, with short rounded cells and tapering hyphal tips The later growth was more regular. (*Text-fig.* 3, 3.)

A similar type of growth was obtained upon bean and prune decoctions.

Attempts to obtain decoctions of *Lemanea* shoot were unsuccessful.

Peptone cultures developed a regular freely-branched mycelium of vigorous growth, with frequent hyphal dilations. (*Text-fig.* 3, 4.)

In all cases a dense weft of mycelium, thinning out to the edges, was ultimately obtained.

Entire perithecia which were placed in hanging drops of culture medium, gave rise to secondary hyphæ, but no spores were ejected; and on subsequently opening the fruiting bodies the contents of the asci were found to have germinated *in situ.*

Cultures in various states of development and vigour were placed under different conditions of temperature, light, and food supply, but sexual reproduction could not be induced.

A large series of experiments were carried out, using a refinement of Woronin's method, and that of hanging drops, in attempting to infect aquatic plants other than *Lemanea.* Hosts supplied were *Oedogonium, Vaucheria, Spirogyra, Cladophora, Chara, Nitella, Riccia, Fontinalis Elodea, Myriophyllum,* and *Ceratophyllum.* The attempts were unsuccessful.

DISTRIBUTION.

From the fact that infected *Lemanea* has been collected during nine months of the year, and in each case all stages of perithecial development and mature spores have been present, it would appear that *L. Lemaneæ* does not shew seasonal periodicity in either vegetative or reproductive development.

The fungus has been found upon *Lemanea torulosa* and *L. catenata,* as well as *L. fluviatilis,* and appears to be not uncommon wherever the alga grows profusely.

PATHOLOGICAL EFFECTS.

Externally the *Lemanea* shows little appearance of disease, the rich deep green colour of the thallus giving place only to a slightly olivaceous hue. Usually the more mature filaments are attacked, and the diseased region may extend from apex to base, the fungus being visible to the naked eye as minute dark brown specks scattered irregularly in the tissue. The presence of hyphæ and fruiting bodies does not usually disturb the general

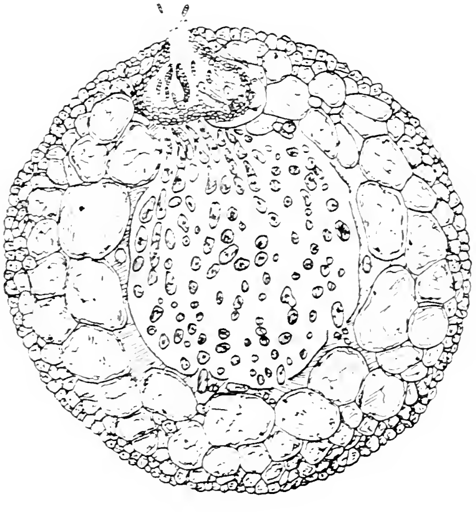

FIG. 4.

Transverse section of infected *Lemanea* shoot, shewing the relative size and position of the fungus fruiting body. The carpospores of the host have developed normally, but the subsequent growth of the perithecium has destroyed the gonimoblastema filaments and procarp.

× 187.

anatomical relations of the host plant. In many instances the perithecia develop in or about the procarp and its attendant structures.

If this occurs prior to carposporic development the latter may be seriously affected or even suppressed; but if subsequently, the spores are usually present in normal abundance. (*Text-fig.* 4.)

Infected filaments not infrequently appear to contain an abnormal amount of mucilage, and it is interesting to note that the fungal mycelium apparently flourishes most vigorously in this substance. The prismatic cortical cells appear to be little affected. In sectional view the difference between healthy and diseased *Lemanea* shoots is striking. The former shew middle and medullary layer cells, with clearly defined walls, and contents consisting of a large central vacuole, a small amount of protoplasm spread thinly around the periphery, and a large nucleus. The latter contains a large, prominent, compact, centrally placed and deeply staining nucleolus, which may be homogeneous, contain one or two vacuoles, or one to four deeply staining granules—"nucleolini." (*Pl. II., Fig.* 19.) In diseased tissue mucilaginous degeneration of the walls occurs, and in many cases it is difficult to determine their boundaries. The fungal haustoria not infrequently appear to apply themselves to the nucleus, which loses its ovoidal form. The nucleolus and nuclear membrane disappear and simultaneously the cell protoplasm degenerates. (*Pl. II., Figs.* 20—22.) No instance has been discovered in which a cell invaded by haustoria has possessed a normal nucleus, and in the majority of cases little or no trace of it remains. Not all the cells which are affected or killed, however, contain haustoria, and on the other hand apparently perfectly normal cells are occasionally present in the midst of affected tissue.

SYSTEMATIC POSITION.

The correctness of the accepted systematic position of the fungus may be questioned. It is included under *Leptosphæria*, a genus in the Pleosporaceæ (**10**), (**13**), (**14**).

This family is characterised by the presence of paraphyses, the 'looseness' of the asci from one another, and the freedom of the perithecium in its later stages; the genus by its yellow to dark brown four-celled spores.

Leptosphæria Lemaneæ, as noted by Woronin, does not possess paraphyses, its asci 'arise together,' and the perithecium never becomes free; whilst its spores are hyaline.

The characters cited would more correctly appear to refer the fungus to the family Mycosphærellaceæ, and the group of genera—*Pharcidia* and *Sphærulina*.

I wish to thank Professor Weiss and Professor Lang for the help they have given me in this investigation.

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PLATE I.

Fig. 1.—Mycelium from the lumen of the host shewing the two types of hyphæ. × 750.

Fig. 2.—Haustorium branching in the vacuole of a generative filament cell. The cell contents have degenerated. × 1,000.

Fig. 3.—Haustorium penetrating the cell protoplasm and sending a branch into the vacuole. Traces of the cell nucleus still remain. × 1,000.

Fig. 4.—Hyphal dilations shewing a dumbbell-shaped appearance. × 1,000.

Fig. 5.—Intercellular hyphal dilations containing two nuclei. × 2,000.

Fig. 6.—Conjugation of a terminal with an intercalary cell. × 3,000.

Fig. 7.—Dilation containing a nucleus of twice the normal size. × 2,000.

Figs. 8, 9, 10.—Uninucleate ascogonia clothed by finer hyphæ. × 2,000.

Fig. 11.—The ascogonium after the first nuclear division. × 2,000.

Fig. 12.—The division of the ascogonium into multinucleate cells. × 2,000.

Figs. 13, 14.—Ascogenous hyphæ shewing asci arising from the binucleate cells. × 3,000.

Fig. 15.—Portion of the base of a mature perithecium shewing the pigmented wall cells, the sterile tissue, ascogenous hyphæ cut across in various directions, and a uninucleate ascus. × 3,000.


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PLATE II.

Figs. 16-18.—Stages in perithecial development.

Fig. 16.—The deeply stained ascogenous elements are surrounded by the sterile thin-walled tissue. × 1,000.

Fig. 17.—The perithecial neck has commenced to form, and the sterile tissue around the ascogenous elements is collapsing. × 1,000.

Fig. 18.—The perithecial neck is formed and the asci have enlarged and become uninucleate. × 1,000.

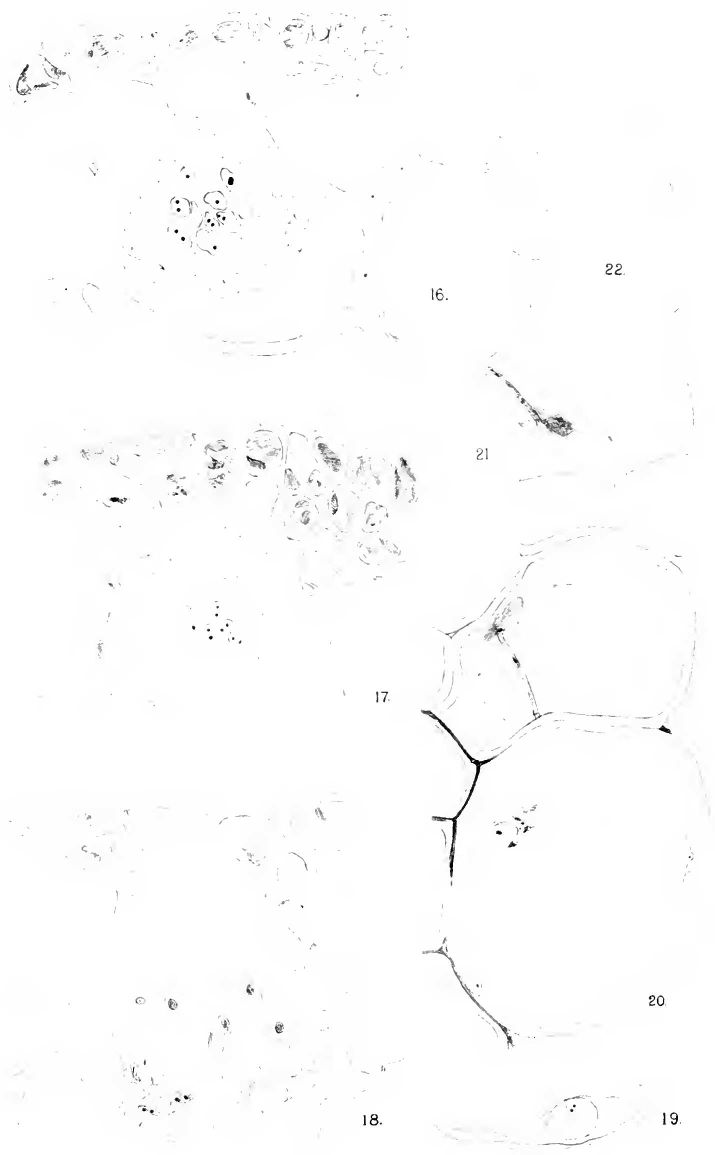
Figs. 19-22.—Stages in the destruction of the host nucleus and cytoplasm.

Fig. 19.—The nucleus of a normal host cell. × 2,000.

Fig. 20.—The nucleus of the lower cell still possesses its nucleolus and nuclear membrane; whilst in the upper cell the former has disappeared and the latter is indistinct. × 2,000.

Fig. 21.—The nucleus has lost both nucleolus and membrane, and the cell protoplasm has degenerated. × 2,000.

Fig. 22.—The cell contents have almost completely disappeared, and the walls are shewing mucilaginous degeneration. × 2,000.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Manchester Memoirs, Vol. lvii. (1913), *No.* 9.

IX. On some abnormal specimens of *Dictyota dichotoma*.

By H. S. Holden, M.Sc., F.L.S.

(*Lecturer in Botany, University College, Nottingham*).

(*Communicated by Professor F. E. Weiss, D.Sc., F.L.S.*)

(*Read May 6th, 1913. Received for publication, June 3rd, 1913.*)

It has long been a matter of common knowledge that many marine algæ are characterised by the fact that their sexual and asexual reproductive organs are normally borne on separate plants, this being a specially noticeable feature in the higher members of the Rhodophyceæ (*e.g.* the Cohorts *Gigartinales*, *Rhodymeniales* and *Cryptonemiales*) and in the *Ectocarpaceæ*, *Cutleriaceæ* and *Dictyotaceæ* among the Brown Algæ. The *Cutleriaceæ* alone, of the forms referred to above, show any differences in the vegetative structure of the sexual and asexual plants: in the remainder the two are, to all appearances, identical.

With the advent of modern cultural methods of study algologists have been able to demonstrate that in many of these algæ there is a fixed alternation of sexual and asexual plants, the fertilised egg on germination producing an asexual plant, the spores of which, in turn, give rise again to a new series of sexual plants, thus affording a close parallel to the conditions obtaining in the Bryophyta and Pteridophyta. Furthermore, this alternation of generations is accompanied by characteristic nuclear phenomena, the nuclei of the cells of the sexual plant, including both ova and sperms, containing the *haploid* (n) number of chromosomes, whilst those of the asexual plant possess the *diploid* ($2n$) number. The nuclei of the spore-mother cells, however, undergo meiotic division, this resulting in a halving of the chromosome number and the consequent restoration of the haploid condition in the next generation. The precision with which these changes normally occur naturally invests any abnormalities with a peculiar interest, and it is with a record of this kind that the present paper is concerned.

During the examination of some preserved material of *Dictyota dichotoma* with a view to the preparation of sections for a senior class, two abnormal specimens were discovered, in which the thallus bore not only well-

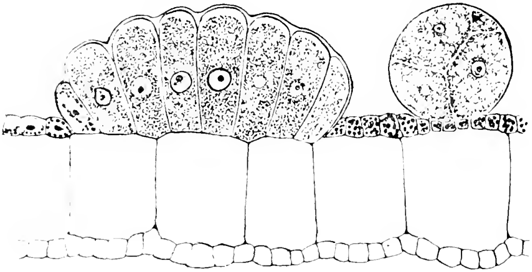

Fig. 1. Section across thallus of *Dictyota dichotoma*, showing the presence of oogonia and tetraspores.

developed oogonial "sori," but also scattered sporangia in various stages of development.

The material was carefully sectioned, but was not sufficiently well preserved to obtain the cytological evidence necessary to settle the various points raised. Future investigation of similar material, with more adequate cytological preservation, will show whether the thallus bearing these two kinds of reproductive organs is *haploid* or *diploid* in character.

Should the former be the case it is probable that the tetraspores are formed without the mother-cell nucleus undergoing meiosis.* In the latter case the egg-cells may possibly develop apogamously, producing a second successive diploid generation.

Lloyd Williams (4) has recorded a feeble apogamous development of the normal oospheres in *Dictyota dichotoma*, (and subsequently (5) in *Haliseris*), accompanied by pathological nuclear phenomena, and he states

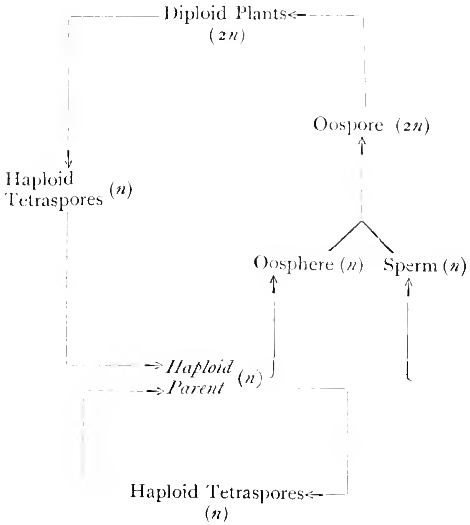

Fig. 2. Diagram illustrating life cycle if parent be regarded as haploid.

* This condition would offer a partial analogy to the case of the abnormal megaspores of *Marsilia Drummondii*, which are produced without meiosis. In *Marsilia*, however, the spores are not haploid but diploid (Bower, (1)).

that "after a few divisions the germlings invariably died." In a previous paper (Lloyd Williams, (3)) the same author has also recorded various abnormalities in tetrasporic plants, among which he refers to the direct germination of the tetraspore mother-cell to form a small mass of parenchyma. This may subsequently "develop an apical cell and grow out into an elongated germling-like branch, and......may remain alive when the rest of the thallus has decayed." Further on in the same paper he states that, as far as his observations go, "this mode of cell-multiplica-

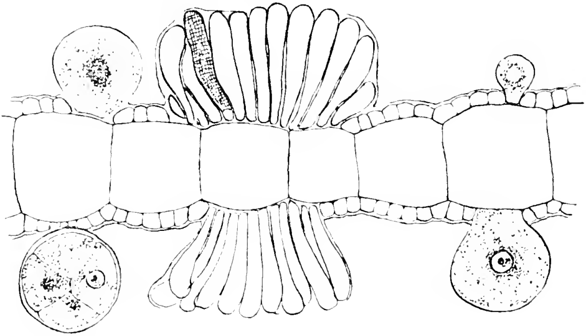

Fig. 3. Section across thallus of *Dictyota dichotoma*, showing antheridial 'sori' and tetrasporangia in various stages of development. The contents of one antheridium are indicated diagrammatically, the scale being too small to show detail.

tion never follows the reduction division." It would thus appear that the nucleus of the tetraspore mother-cell does not invariably undergo meiotic division, but may give rise to a sort of diploid gemma-like shoot.

In the case under consideration there is, however, a certain amount of indirect evidence which tends to show that the parent plants were haploid and consequently that the tetraspores were produced without chromosome reduction, and, if functional, would give rise to a second haploid generation. (*Vide Fig.* 2.)

This evidence was in part provided by a further supply of material from Plymouth, which, besides containing another plant with oogonial "sori" and tetraspores, also furnished four specimens in which *antheridia* and tetraspores were associated on the same thallus (*Fig.* 3). The very fact of the existence of antheridial material, and also its association with that providing oogonia, seems to suggest that the sexual organs functioned normally.

This view also receives some support from the occurrence of similar abnormalities in the Florideæ. I have observed both tetraspores and antheridia, and tetraspores and all stages of cystocarpic development on the same plant of *Polysiphonia fastigiata* and on a species of *Ceramium*,* so that here at least the sexual organs are obviously functional.

Similar cases are recorded by Yamanouchi (**6**) as occasionally occurring in *Polysiphonia violacea*, and he suggests that the four cells derived from the tetraspore mother-cell behave as monospores; in other words, the nucleus of the mother-cell, being haploid, does not undergo meiotic division, but produces a second series of haploid daughter plants. He thus adopts the view stated here as the most probable for *Dictyota*. Harvey-Gibson (**2**), in a short paper dealing chiefly with additions to the list of marine algæ in the L. M. B. C. area, also refers to similar abnormalities in *Ectocarpus confervoides* among the Phæophyceæ, and in *Chylocladia kaliformis*, *Callithamnion baileyi*, *Spermothamnion turneri* and a number of other Florideæ, and states that among the plants of *Polysiphonia*

* These were glycerine-jelly mounts of material collected at Aberystwyth.

violacea collected by him at Port Erin the production of sexual and asexual reproductive organs upon the same plant is by no means rare.

I understand that the cytology of these forms is at present being studied in the Hartley Botanical Laboratories, Liverpool, so that we may hope, in the near future, for a definite solution of the fascinating problems raised by their occurrence.

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X. The Variation of *Planorbis multiformis*, Bronn.

By George Hickling, D.Sc., F.G.S.,
Lecturer in Palæontology in the University of Manchester.

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Among a collection of fossils which recently came into my possession was a mass of shell-limestone composed mainly of shells of the above-named species. The source of the material was unfortunately not recorded. Upwards of five hundred shells of the *Planorbis* had fallen free from the matrix, most of the individuals being indistinguishable from the various forms from the Miocene of Steinheim, so amply figured by Hyatt. ("The Genesis of the Tertiary Species of *Planorbis* at Steinheim." Anniversary Mems., Boston Nat. Hist. Soc., 1880.) In some individuals the spire attains a still greater height than in any of Hyatt's examples.

The occurrence of all the specimens here considered in a single lump of material naturally raised the question whether they were all referable to a single species. If the photographs of the accompanying plates are compared with those of Hyatt's plate 9, it will be seen that the shells now described include representatives of several of Hyatt's species and sub-species, some of which are stated to be confined in the Steinheim deposits to distinct horizons. In my own material it was readily noticeable that forms with moderate spires were abundant, while extreme forms—*i.e.*, those which were flat and those which had high spires—were rare. This would obviously be the case if we were dealing with an example of extreme continuous variation of a single species. On the contrary, if several species or sub-species were present, there would be no reason why the various types should be represented in any definite proportion. It therefore appeared desirable to ascertain exactly the relative numbers of individuals of various types, since the variation-curve so obtained would give the most decisive possible answer to the question of the existence of one or more specific units.

The problem of constructing a variation-curve is complicated by the fact that the shells vary independently in several characters, of which the height of the spire is merely the most conspicuous. The existence of these concomitant variations makes it almost impracticable to apply any system of measurement to the shells, while an elaborate statistical enquiry is neither necessary nor justified by the relatively small amount of material available. It was therefore deemed sufficient to investigate the variation in the "height-ratios" of the shells

$$\left(i.e.,\ \text{the ratio}\ \frac{\text{height of shell}}{\text{diameter of base}}\right)$$

by sorting the shells in respect of this character into ten grades by eye-estimation. The first step was to select ten typical shells, differing in height as nearly as possible by equal amounts, to form a scale of comparison covering the whole range of variation. The whole of the material was then sorted into ten grades by comparison with this scale.

The shells examined were of various stages of growth, and they were accordingly divided initially into three groups, according to size: in the smallest, the diameter of the base is from 0·08 to 0·12 inch; in the median set, 0·12 to 0·20 inch; in the largest, 0·20 to 0·28 inch. This was done to facilitate both the sorting and the interpretation of the results. A similar type set of comparison-shells was made for each of the three grades of size. Since it is almost impracticable, as already indicated, to obtain numerical values for the character here considered, the successive grades have merely been numbered A to J in order of increasing height-ratio. In the subjoined curves (*Fig.* 1) the results of this enquiry are plotted, height-ratios being taken as abscissæ, numbers of individuals as ordinates.

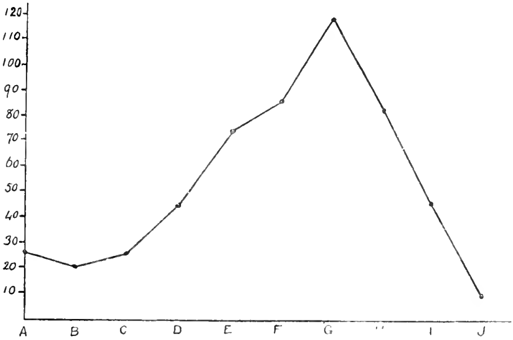


Fig. 1. Curve showing distribution of "height" $\left(=\frac{\text{height}}{\text{diameter of base}}\right)$ among 533 individuals of *Planorbis multiformis*. Some of the individuals were not full-grown. The heights represented by the letters A-J may be seen by reference to *Pl. I., Fig.* 1.

The Variation Curves: Biological unity. In *Fig.* 1 is shown the curve representing the frequency of the "height" character in all the shells (533) examined. The group A includes the "lowest" shells (those actually discoidal), while group J represents the "highest" forms. The type-set of shells is shown in *Pl. I., Fig.* 1. Inspection of the curve at once reveals the fact that the numbers of individuals falls off very regularly as the height of the shell departs from the mean in either direction—and considering the small number of shells dealt with, and the necessarily crude method by which they were sorted, the regularity of the curve is very striking. It is a typical "continuous variation" curve, or "curve of error," such as would represent the frequency of height or of length of span in man.* If the shells of different heights represented different species, or races, or "mutants," the chances against the various forms being present in the regularly distributed numbers in which they are actually found would be almost infinitely remote. On the contrary, the simple variations of any character in a single organic type are necessarily distributed in these proportions, unless disturbed by the selective action of external factors. Such a variation-curve is the most conclusive proof possible that the group of individuals from which it is derived belong to one indivisible organic unit, *i.e.*, form one species in the strictest sense. Systematists may apply distinctive names to different forms for convenience of description if they choose, but these divisions have no objective existence. In passing, it may be remarked that this method of enquiry would afford a most satisfactory means of settling disputed questions of the specific unity or otherwise of any series of organisms.

There appears to be one case only in which the evidence of such a curve is not conclusive. It is possible to get a number of related species which present exactly similar variations in respect of a given character, and in such a case the curve for that character will not be affected if the species are mixed. Such cases occur both as the result of convergence produced by a common environment and as the effect of parallel development from a common ancestry. It will be shown in the sequel

* Certain peculiarities of the curve are dealt with below.

that the present case is certainly not of that character. The primary conclusion of this enquiry, therefore, is that the wide range of forms of *Planorbis multiformis* here described all belong to a single species in the strictest sense.

Ontogeny: Variation in height with age. As already explained, before the shells were classified according to

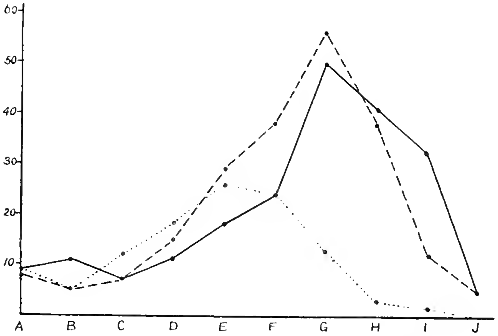


Fig. 2. Curve similar to *Fig.* 1, but with the component individuals classified according to size.

Dotted line = individuals with base diameter of 0·08-0·12 inch.

Broken line = individuals with base diameter of 0·12-0·20 inch.

Continuous line = individuals with base diameter of 0·20-0·28 inch.

height, they were divided into three groups according to size. The curves obtained for each of the groups separately are shown in *Figs.* 2 and 3. *Fig.* 3 differs from *Fig.* 2 only in the fact that the curves have been slightly smoothed by summing the successive terms in overlapping pairs.

For each set of shells we obtain a simple-variation curve, thus confirming the conclusion from the general curve that we are dealing with a single species. But the three curves differ strikingly in the fact (most obvious in *Fig.* 3) that the maximum or "mode" of the curve is displaced in the direction of increasing height with each increase in size. In other words, throughout life, as the shell grows, the ratio *height : diameter of base* increases. This fact would be much more striking if it had been possible to construct a curve for shells less than ·08 in diameter, for these would fall almost entirely into classes

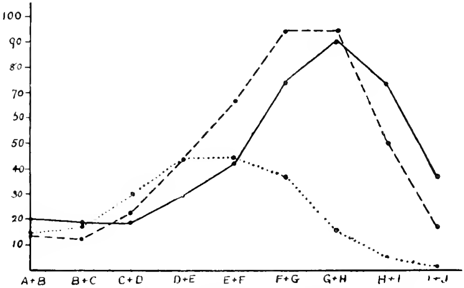


Fig. 3. Curves as in *Fig.* 2, but smoothed by taking the sums of overlapping pairs.

A, B and C, with the maximum in B. Unfortunately I had not a sufficient number of specimens so young, but those at my disposal clearly indicate what the result would be, while it is readily confirmed by an examination of the apical portions of the adult shells. By the last-named method of enquiry it may be shown that every individual begins life as a discoid shell—a typical *Planorbis*. In a general way, therefore, these changes in the form of the variation-curve as the shells increase in size is merely a graphic representation of the fact that the young shell is a discoid *Planorbis* which acquires a turreted character during growth, a change which must, without doubt, be regarded as an ontogenetic recapitulation of its phylogeny. But it is very important to notice, first, that the change occurs quite gradually; secondly, that it is continued throughout the adult stages. The first observation carries with it, as a necessary corollary, that the increasing height has been acquired during the past history of the species as a similar gradual process. There can be no question of the turreted form having arisen as a "mutation," else some trace of the fact must be preserved in the ontogeny. The continuation of the changes throughout life is open to a variety of interpretations, but at any rate shows that there is no defined form to which the species attains.

During growth, the turreted character is thus gradually assumed by each individual, but to varying degrees, whereby the great variety of adult forms arises. In addition, however, to the varying degree in which this character is finally expressed, there is an *independent* variation in the rate at which it is assumed by different individuals. That is to say, the height of the shell at a given stage of growth cannot be taken as a measure of the height to which it will attain when fully grown. This is best seen by taking a group of adult shells of about mean height (form G) and examining their apices. It is then found that some have a much flattened apex, due to the fact that the shell maintained its discoid mode of growth during the formation of several complete whorls. In other cases the apex is much "sharper" (*i.e.*, more conical), owing to the much earlier assumption of the turreted condition. Such cases are shown in *Pl. II., Figs.* 5 A-C, 6 A, B. In *Figs.* 6 C, D, of the same plate similar variations are shown in much flatter shells. They occur, in fact, in shells of all heights, except purely discoid types, which cannot, of course, express this variation.

The variation in the rate of assumption of the turreted condition may be seen best of all by cutting the shells in median section. The adjoining *Figs.* 4 and 5 are camera-lucida drawings of the apical portions of two such sections

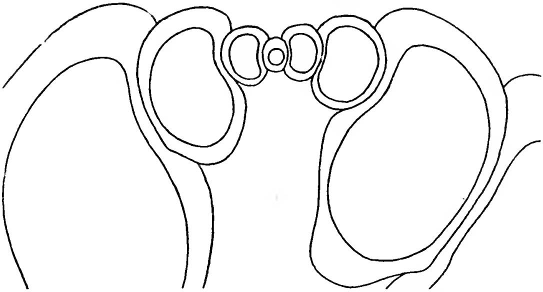


Fig. 4. Section of apex of shell of mean height, showing smooth discoid young stage with wide umbilicus.

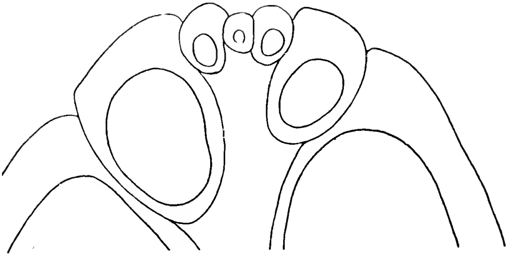


Fig. 5. Section of apex of shell of mean height, showing rapid assumption of turreted character and early narrowing of umbilicus. Compare with *Fig.* 4.

of shells which were of exactly the same height.* The contrast between the form of the young stages, at the completion of the third whorl, is extremely striking.

* The shells were exactly similar to those of *Pl. II., Fig.* 5 A, B. *Fig.* 6 is a section corresponding to *Pl. II., Fig.* 5 C.

Now it is evident from a careful examination of a large number of specimens that shells with a conspicuously flat apex are rare, as are also those with a very pointed apex, while intermediate types are common, whence we are justified in the conclusion that the variation in the rate of assumption of the turreted character is again a continuous variation.

It is, of course, obvious that this variation must be evident from an examination of the young shells themselves, but it will be convenient to defer this enquiry

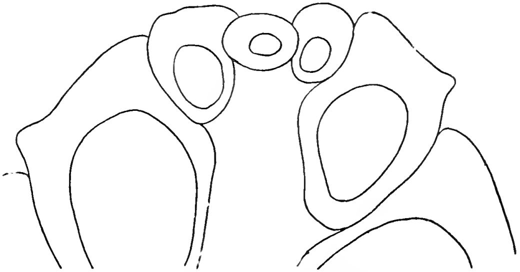


Fig. 6. Section of apex of shell of mean height, showing early development of very strong carination.

until some of the other variable characters of the shells have been considered.

Variable Characters other than Height.

(1) *Carination.* The shells may possess two carinæ (*i.e.*, ridges running along the coils of the shell), a dorsal or median one, and a ventral one; either or both may be absent. These carinæ are usually moderately developed, occasionally being very strong or rarely quite absent. Between these limits every variety may be found, as shown on *Pl. I., Fig.* 2 A-D, E-R, I-L. They are, therefore, contin-

uous variations. It is shown by the figures just mentioned that the development of the dorsal carina is quite independent of the height of the shell. The ventral carina, however, shows a certain correlation with that character. It is never strongly developed in "high" forms. In "low" forms it shows the same variations as the dorsal carina.

As regards its ontogeny, the carination exhibits three conditions, as follows:

(*a*) The carina may be strongly developed at an early stage and may remain in that condition throughout the growth stages.

(*b*) The carina may be strongly developed at an early stage and then be gradually lost.

(*c*) The carina may be feebly developed at an early stage and may remain so throughout life.

The significant fact, therefore, is that feebly carinate adults may develop from strongly carinate young, while the reverse appears never to occur. It may be added that the last whorl of an adult shell generally shows decreasing carination. These facts, taken together, make it fairly clear that this character is now in a catagenetic condition.

The first whorl of the shell is, nevertheless, invariably quite smooth, showing that they have developed from a non-carinate ancestor. Many individuals thus show the complete cycle of the growth and decay of the carination.

(2) *Involution.* The extent of overlap of the successive whorls shows well-marked, though not very extensive variation. In a "normal" shell the suture-line coincides exactly with the dorsal carina, but in other cases it may lie a little above or a little below this line. The amount of variation in this respect is well shown by *Pl. I., Fig.* 3 A-E. This character shows no marked relation to the height of the spire, the "normal" position of the suture being on the dorsal carina in all cases, while similar varia-

tions occur in shells which are almost discoidal, and in those with high spires. Not only does variation occur among different individuals, but considerable changes may take place during the growth of a single individual, as in the example shown in *Pl. I., Fig.* 3 E, where the apical part of the shell is almost normally involute, while the lower portion is much less involute. Similarly, the apex may be sub-normally involute, while the later parts are normal, and so on. Marked variations of the type just described within the individual are relatively infrequent. Another individual variation in involution is almost constant, namely, the terminal portion of the last whorl of the mature shell is nearly always somewhat "turned down," so as to be less involute than the preceding portions.

(3) *Shoulders and Channelled Sutures.* These features vary generally in relation with the degree of involution. When the involution is normal, the upper flank of each whorl is nearly flat, with its edge only very slightly incurved where it joins the whorl above (see, *e.g.*, *Pl. I., Fig.* 3 C). In those cases in which the overlap of the whorls exceeds the normal, the sutural margin of the upper flank of each whorl is strongly incurved, producing a "channelled suture," while the adjacent portion of the flank forms a strongly arched, prominent "shoulder" (see, *e.g.*, *Pl. I., Fig.* 3 A and B). The upper flank of the whorl, between the shoulder and the dorsal carina, is in these cases slightly concave in form. On the contrary, when the overlap is less than normal, there is generally no shoulder or channelling, and the upper flank tends to be convex (see *Pl. I., Fig.* 3 D). The relation of these variations to the involution, though general, is by no means constant. It should be noticed that the form of the upper flank of each whorl is modified not only by the

variations in shouldering and channelling, but also by the degree of development of the dorsal carina.

(4) *Umbilicus.* In the early stages of growth, the width of the umbilicus varies greatly, according to the length of time during which any individual maintains a more or less discoid habit. The differences which arise from this cause may be readily appreciated by a comparison of *Figs.* 4 and 5. In the adult shells there is likewise a general tendency for the umbilicus to become more narrow among shells with a high spire than among those of a "lower" form; but, in addition to this variation with height, the umbilical width in the mature shells varies considerably as a result of the varying form of the base of the whorl. This latter variation, among shells of the same height, is shown in *Pl. I.*, *Fig.* 2 A-D, though these shells were not selected for this purpose. It is evident that all the variations in umbilical width are continuous in character.

(5) *Aperture.* The form of the cross-section of the whorl is affected by all the variations already noticed, and, in addition, the actual aperture of a completely mature shell may present further independent modification. The changes in apertural form with height are seen in *Pl.* I., *Fig.* 7. The flattest adult shells always have the whorls strongly quadrangular in section, corresponding with a strong development of both dorsal and ventral carinæ. As the spire becomes higher, the aperture tends to become pear-shaped, the change in form being perfectly gradual. Carination always affects the form of the aperture, producing more or less angularity in its outline. When a shoulder and channelled suture is developed, the aperture shows a well-marked superior sinus corresponding to the shoulder. Finally, the base of the whorl varies considerably as regards the degree of flattening or round-

ing which it exhibits, and the form of the aperture varies accordingly.

In addition to such modifications dependent on other variable characters of the shell, the terminal portion of the last whorl of the mature shell is frequently deflected in a downward direction, being at the same time slightly enlarged and less extensively in contact with the preceding whorl. The aperture is hereby increased in size and made less angular. In extreme cases these changes may be carried to the extent of a very small portion of the last whorl, becoming quite free and almost completely circular in section. It is perhaps justifiable to regard this tendency to an ultimate decrease in involution with increased circularity of section as a gerontic reassertion of the ancestral type of the shell. The important fact, however, is that, whatever significance may be attached to any of the various modifications in form of aperture, all the changes of this character, as of all the other characters of the shell, take place by insensible gradations, and so cannot be made diagnostic of any subdivisions of the species which it might be desired to erect.

Variations in the Form of the Young Shell. By far the most surprising fact with regard to this remarkable species is the circumstance that at a very early stage of growth the shells assume a remarkable diversity of form, while these diverse young may develop into adults, which are almost indistinguishable as regards their later-formed portions. Shells no more than 1 mm. in diameter may appear so dissimilar that under ordinary circumstances they would certainly be regarded as distinct species; yet they may develop into quite similar adults. Were isolated specimens only available for study, this would naturally be regarded as a case of heterogenetic homœomorphy—*i.e.*, the diversity of the young stages would be taken as an indication that the shells were the descendants of several distinct ancestral types, which had become similar by the acquisition of similar modifications. Hyatt did, in fact, to a limited degree, draw this conclusion, though the phenomenon of homœomorphy was not adequately understood at the time he wrote. Nevertheless, the explanation is not applicable to the present case, for, as a reference to *Pl. II., Fig.* 4, where a series of young forms is photographed, will show, all the types of young pass insensibly into one another. In other words, the young stages represent, not several distinct species, but a single highly variable ancestral form.

The actual variations of the young shells appear to be entirely due to the development, in different degrees, of carination. This may be completely absent (*Pl. II., Fig.* 4 A), giving the "*rotundatus*-like young" of Hyatt. In others the median carina appears alone, and finally both carinæ may be present. As the two carinæ become more strongly marked, the section of the whorl becomes correspondingly more rectangular. There can be little doubt, either on general grounds or from a consideration of the actual geological history of these shells as recorded by Hyatt, that the smooth form is the ancestral one. Further, the fact that all stages of carination appear in young shells which are yet completely discoidal is a sufficient proof that in phylogeny carination was acquired before turreting.

GENERAL CONCLUSIONS AND DISCUSSION.

The main conclusions here reached with regard to *Planorbis multiformis* are:—

(1) The ancestral form of the shell was discoid and smooth, with whorls of rounded section.

(2) Carination was subsequently developed, producing a complete range of discoid types, varying from the most strongly carinate, with whorls of rectangular section, to the ancestral non-carinate form.

(3) Later variation gave rise to turreted forms, this character being also developed in every degree, and arising in all the pre-existing varieties.

(4) Throughout all these changes, the whole group of shells has maintained complete biological unity, as shown by the fact that the co-existing ultimate forms here described have all their varieties connected by a typical "continuous variation" curve, and must therefore be regarded as constituting a single species.

The simplest conception of the species as here described may be expressed by defining it as a *Planorbis* which has become potentially carinate and turreted, either of which potential characteristics may be inherited by the individual in any degree, the frequency of the inheritance following the law of error.

The minor variations described above may all be regarded as essentially consequences of these two principal modifications, separately or in combination, and, for the sake of clearness, may be neglected in a general discussion. With this restriction it is evident that four types of *Planorbis multiformis* potentially exist :—

(1) Non-carinate discoid.
(2) Carinate discoid.
(3) Non-carinate turreted.
(4) Carinate turreted.

Further, it has been shown that carination is now in a declining condition, since individuals which are carinate in an early stage may become smooth on further growth. Hence it follows that two other types, which may be termed "post-carinate," might theoretically occur, viz.:—

(5) Post-carinate discoid.

(6) Post-carinate turreted.

Of these six types, four are actually realised among the adult shells here described, types 1 and 5 being absent. The first type occurs among the young shells, but never retains its primitive form to maturity—in other words, the unmodified ancestral form has ceased to exist. The absence also of the fifth type seems to indicate an antagonism between a discoid form and smoothness, which may explain the non-survival of the primitive form. Possibly the uncarinate discoid condition is mechanically weak.

In this connection, a peculiarity of the variation-curves for height may be considered. It is clear that the curves for the adult shells are asymmetrical. While the greatest number of individuals have a moderately high spire, the proportion of those with low spires, and especially of the discoid types, is notably higher than it should be on the hypothesis of pure simple variation about the mean. In fact, the curve shows a tendency to rise again on reaching the perfectly discoid forms, though unfortunately the number of shells available is much too small to make this peculiarity of the curve certain. The point is worthy of further investigation with more material, since it would indicate that, although the series of forms here considered is undoubtedly continuous, there is a tendency for the discoid type to separate off as a distinct form. However that may be, there can be no doubt as to the general asymmetry of the curves, which may best be translated into the statement that there is a much greater variation in height among shells with spires lower than those of the commonest type than is found among shells of greater height. The significance of this fact must be considered in relation with the geological history of the shells.

It will probably have seemed to many that the occurrence of the whole range of shell-forms here described in a single mass of material is incompatible with Hyatt's assertion that the same series of forms at Steinheim constitutes a stratigraphical sequence, in which the discoid types occur at the lowest and the high-spired forms at the highest horizons. Nevertheless a perusal of his paper and a consideration of the manner in which his material was collected and investigated must result in the conviction that his assertion is substantially correct. His statement is not, however, that each type occurs at one horizon only, but merely that the discoid forms alone are found at the lowest horizons, while the turreted types appear later and finally predominate. Now this is just what our study of the present material has led us to anticipate, viz., that the species was originally discoid, a turreted tendency having subsequently appeared and gradually increased in degree, so that the most recent form is one with a fairly high spire. Could this form be isolated, it would doubtless show a comparatively small range of perfectly symmetrical variation. Among the actual shells, however, each of the preceding types still finds expression, but the frequency of the occurrence of each type would seem to be inversely proportional to the remoteness of the type in the ancestry of the existing forms, presumably according to an exponential law. This hypothesis at least appears to give the simplest and most satisfactory explanation of the facts concerning the distribution of the existing varieties.

The alternative is to assume that the maximum frequency as determined by inheritance should occur among shells of truly mean height, and that the observed preponderance of "high" forms is the result of a selective death-rate during growth. This involves the assumption of an enormous range of purely simple variation, for which no explanation can be offered, while it takes no account of the geological history of the species. The hypothesis here put forward might be tested by an accurate statistical investigation to determine the exact form of the variation curves. Indeed, this suite of shells would seem to offer an exceptionally favourable field for the statistical investigation of a variety of problems connected with variation and inheritance.

A few other points seem to require brief references. Those who are familiar with Hyatt's paper will have recognised that the forms here described represent only the third and fourth of the four genetic series which he has constructed—possibly the fourth only, though I think the third should be united with it. No representatives of his first and second series are present in my material, and I am therefore not in a position to discuss the relations of these. The earlier members of the third and fourth are likewise unrepresented *by adult shells.* His fourth series begins with *Planorbis Steinheimensis*, a form closely in agreement with the theoretical ancestor which ontogeny had led us to anticipate—the "non-carinate discoid" type. It has been shown that among the shells here considered this type no longer exists, though it is practically represented by the young stages of some individuals. Nevertheless the material does actually contain a few shells which are either *P. Steinheimensis* or a very slightly modified form derived from it (*Pl. II., Fig.* 8); but these do not belong to the series. There are absolutely no intermediate forms to connect them with the others, *except among the extremely young shells.* Here, then, we seem to have a type which has now definitely separated off as a distinct species, the ancestral form having parted company with the carinate and turreted varieties. The partition in this case serves to emphasise the united character of the remaining forms. Of far greater importance, however, is the fact, as it appears to me, that by such partitions, and in no other way, can a species be defined. Herein lies the whole distinction between the treatment of these shells now given and that adopted by Hyatt. He assumes that any form recognisably distinguished from another may be designated a distinct species. On this assumption any person is at liberty, among a series of forms, to distinguish as many or as few species as he chooses, since the "species" is an abstraction with no objective existence. But the aim of classification is to indicate the branches of the zoological tree, of which the ultimate branches are species. In other words, no form can rank as a species until it becomes biologically independent. It has already been hinted that the strongly carinate discoid forms here described seem to show a tendency to break off as the earlier type has done, but until a definite discontinuity appears, they cannot, in my opinion, be regarded as constituting a distinct species.

Two observations may be made as to the bearing of this study on the use of palæontological data for stratigraphical purposes. Firstly, as we have just seen, a series of organisms may form an evolutionary chain and appear in stratigraphical sequence, yet the members of the series may not be diagnostic of successive zones, owing to the persistence, as here described, of the earlier types, conjointly with their modified descendants. In such a case, the horizon clearly cannot be determined, except by a complete suite of specimens, sufficient to determine the real stage of evolution reached. An individual specimen will only serve to determine that the horizon cannot be earlier than that at which the type represented makes its first appearance. Incidentally, there should be noted the importance of studying the *proportions* in which the several variants of a species are present in a given stratum.

In conclusion, I would point to the striking testimony borne by the history of these shells to the production of extensive morphological changes by a process of purely continuous variation. In this case it is clearly quite impossible to introduce any idea of "mutation," at least in the de Vriesian sense, into the process. No one can indicate the point at which a new "form" or "type" or "mutant" appears, the sequence being quite unbroken. And surely the great mass, if not the whole weight, of palæontological evidence points in the same direction? Nothing, it appears to me, could be more dangerous, or more subversive of the logical basis of stratigraphical palæontology, than the introduction of the idea of mutation. If species or varieties arose in that manner we must assume that the date of their appearance can be defined with absolute precision, since there can be no fine stages of modification leading gradually up to them; if the contrary is true, there can be no absolute palæontological boundary lines. It must be long before the relative importance of these two factors in evolution can be finally settled, but in the meantime one case in which continuous modification is clearly demonstrable as a palæontological fact should outweigh many instances in which new types appear without ascertained intermediates linking them to their parent species.

DESCRIPTION OF PLATES.

All the photographs represent varieties of *Planorbis multiformis*, except photo 8.

PLATE I.

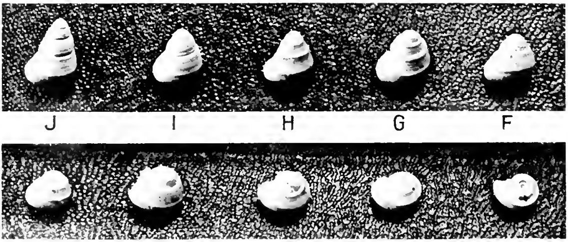
Fig. 1.—Complete set of forms showing variation in height. This is one of the type-sets used as a standard of comparison in sorting, as explained in the text. The shells are full grown. × 1·4 dia.

Fig. 2.—Three sets of mature shells showing variation in carination. Sets A-D and E-H are of height B (*Fig.* 1) and show the ventral and dorsal carinæ respectively. The shells I-L are of height G (*Fig.* 1) and show the dorsal carina. The ventral carina is faintly seen in J, but is never strongly developed in "high" shells. × 2·3 dia.

Fig. 3.—Series of mature shells showing variation in degree of overlap (involution) of the whorls. In A and B, note the "shoulder" and "channelled suture" which arise when the overlap involves the dorsal carina. × 2·3 dia.

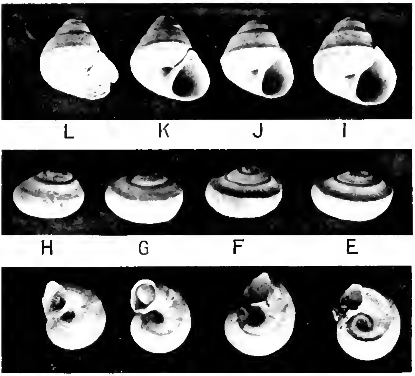
1

×1·4



2.

×2·3



3.

×2·3

Bemrose, Collo., Derby.

PLATE II.

Fig. 4.—Series of very young shells showing the variation produced by the development or otherwise of carinæ. The upper and lower rows represent the same shells. A is the primitive smooth form. It will be seen that the dorsal carina develops first, the ventral one later. Compare with *Text-figs.* 4-6. × 3·7 dia.

Fig. 5.—Three mature shells of height G (*Fig.* 1), showing variation of apex. Compare with preceding figure. Apex of A = A or B of *Fig.* 4; apex of B = D of *Fig.* 4; apex of C = F or G of *Fig.* 4. × 3·7 dia.

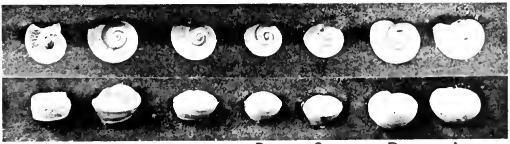
Fig. 6.—Mature shells showing variation in form of apex, in high (A, B) and low (C, D) forms. × 2·3 dia.

Fig. 7.—Series of mature shells showing variation in form of aperture. × 1·7 dia.

Fig. 8.—*Planorbis Steinheimensis* (?) associated with the foregoing shells. These forms agree almost completely with *Fig.* 4 A except in size, but are not linked by any intermediates to the mature discoid forms of *P. multiformis.* × 3·7 dia.

4.

×3·7



G F E D C B A

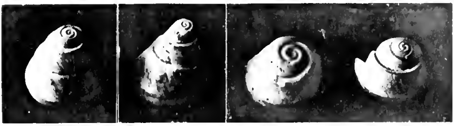
5.

×3·7

A B C

6.

×2·3



A B C D

7.

×1·7

8.

×3·7

Bemrose, Collo., Derby

Manchester Memoirs, Vol. lvii. (1913), *No.* **11**.

XI. On some relations between *Puccinia malvacearum* (Mont.) and the tissues of its host plant (*Althaea rosea*).

By WILFRID ROBINSON, B.Sc. (Lond.).

(Communicated by Professor W. H. Lang, M.B., D.Sc., F.R.S.)

(Read April 22nd, 1913. Received for publication June 6th, 1913.)

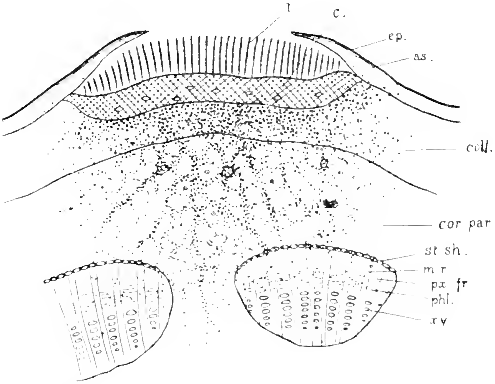
This paper deals with some features of the Hollyhock rust, the disease caused by *Puccinia malvacearum*, and especially with the relations between the tissues of the host and the parasitic fungus. This disease is common in market gardens near Manchester, and experimental work on the conditions of infection is in progress. A more detailed knowledge of the histological features of the diseased regions was a necessary preliminary, and the present paper deals with this portion of the work.

Puccinia malvacearum has been studied by a number of investigators, and the main features of its life-history are well known. It was first described by Montagne in 1852, and since that date Kellerman, Rathay, Taubenhaus, Eriksson and others have studied its morphology and life-history from various points of view. It occurs chiefly on *Althaea rosea* and *Malva sylvestris*, but has been found to attack many other members of the Malvaceæ. The sori of teleutospores occur abundantly on the lower surfaces of the leaves and also on the petioles, stems, bracts, sepals and even on the young fruits. On the leaves the sori are at first circular but often become confluent, whilst on the stem and petiole they are elongated and elliptical in outline. The teleutospores, on reaching maturity, germinate on the plant, and under normal conditions produce pro-mycelia each bearing four sporidia. The fungus is an autœcious member of the Uredineæ, which produces teleutospores only, and it has therefore been classified in the sub-group Lepto-puccinia (Schröter).

Little has been done on the intimate relations between this fungus and the cells of the host. Attention has therefore been chiefly devoted to this, and to the histological features of the diseased spot as compared with the corresponding normal tissues. It will be convenient first to describe the features of pustules on the petiole, stem and leaf blade respectively, and then to describe the relations between the individual cells of the host and the haustoria of the fungus.

The general relations of the fungus to the tissues of the host plant are represented diagrammatically in *Text-figs* 1-4. In the figures the distribution of the several tissues of the host is mapped out and the area occupied by the mycelium of the fungus is indicated by dots. The mass of hyphæ beneath the sorus is indicated by cross hatching (*c*) and the sorus of teleutospores by vertical lines (*t*). These figures, though diagrammatic, are in each case founded on actual sections through characteristic pustules.

Text-fig. 1 represents a portion of a transverse section through a petiole, with a pustule in a moderately advanced stage of development. The petiolar structure is of an ordinary dicotyledonous type. There is an epidermis (*ep.*), a layer of assimilating tissue (*as.*), a band of collenchyma (*coll.*), a cortical parenchyma of large cells (*cor. par.*), and a ring of vascular bundles two of which are indicated. Cells containing starch occur around the vascular bundle and form a definite starch sheath (*st. sh.*) to the outside of the latter. The general distribution of the fungus in the tissues is shown in the figure. The sorus of teleutospores (*t*) rests upon a definite mycelial cushion (*c*), which has evidently been formed by the occupation of the intercellular spaces by larger masses of hyphæ, leading to the disorganisation of some of the cells of the collenchyma. The hyphæ extend inwards in the intercellular spaces of the cortical parenchyma, making a very definite attack on the starch sheath. Strands of mycelium pass into the vascular bundle by way of the

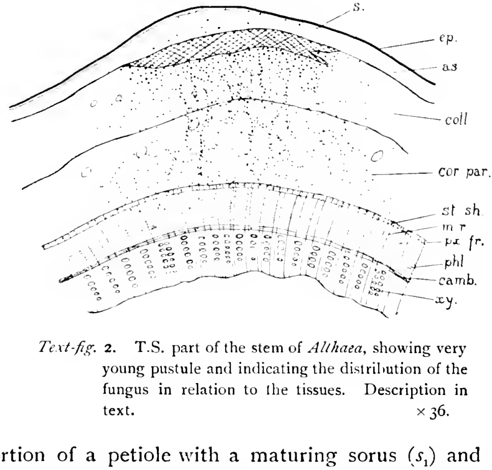


Text-fig. 1. T.S. part of the petiole of *Althaea rosea*, showing young pustule with relations of mycelium to host tissues. Description in Text. × 36.

less resistant phloem rays (*m.r.*), and hyphæ then ramify among the tissues of the phloem (*phl.*). Branches of the mycelium extend to the parenchyma within the circle of bundles. This distribution of mycelium suggests a definite absorptive system of hyphæ in relation to the individual pustule.

In the stem (*Text-fig.* 2) the distribution of the fungus in the tissues is similar, except that on account of the complete ring of vascular tissue the mycelium only rarely reaches the pith. As in the petiole, there is a definite attack on the starch sheath, medullary rays, and the phloem elements of the vascular bundle.

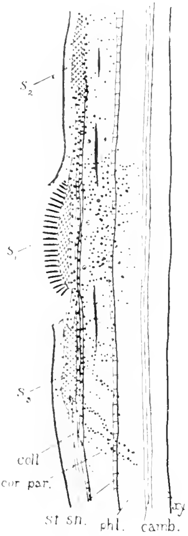
On the stem or petiole several pustules are often found in close proximity. Whilst many of these may represent separate infections, some are subsidiary sori that have originated as off-shoots from the main sorus. *Text-fig.* 3 represents a longitudinal section through a



Text-fig. 2. T.S. part of the stem of *Althaea*, showing very young pustule and indicating the distribution of the fungus in relation to the tissues. Description in text. × 36.

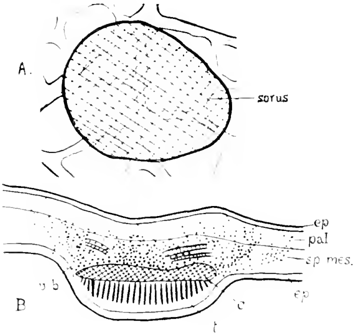
portion of a petiole with a maturing sorus (s_1) and two younger subsidiary sori (s_2, s_3) borne on the same mycelial system.

On the leaf blade the sori occur most frequently on the lower side, that is, with the teleutospores directed downwards. *Text-fig.* 4A, which is a portion of the leaf blade seen in surface view, shows that each sorus is related to several of the finer veins of the leaf. Stomata occur on the upper and lower surfaces in the ratio of 4 to 7. Since, however, the sporidial germ tube penetrates the epidermal wall directly, and does not enter through the stoma, the fact that the majority of the pustules appear on the lower surface is not to be directly explained by the greater abundance of stomata there. Inoculation



Text-fig. 3. L.S. portion of stem, showing small mature pustule s_1 and two subsidiary pustules s_2 and s_3. Description in text. × 20.

experiments showed that even when the sporidia were sown on the upper surface of the leaf most of the sori resulting were on the lower surface. The general distribution of the fungus in relation to the tissues of the leaf is shown in *Text-fig.* 4B. The young teleutospore sorus (*t*) is situated on a mycelial cushion (*c*) directly below the epidermis (*ep.*), and the mycelium ramifies in the intercellular spaces and disorganised tissues of the mesophyll right through to the upper epidermis. At maturity the developing sorus ruptures the epidermis, as in the case of the petiole. These observations confirm the descriptions of Taubenhaus[1] and of Werth and Lugwigs,[2] but whilst



Text-fig. 4A. Surface view of leaf lamina, showing pustule in relation to several of the finer veins. × 50.

B. V.S. of portion of leaf lamina bearing a young pustule and showing the distribution of the fungus in relation to the tissues. × 50.

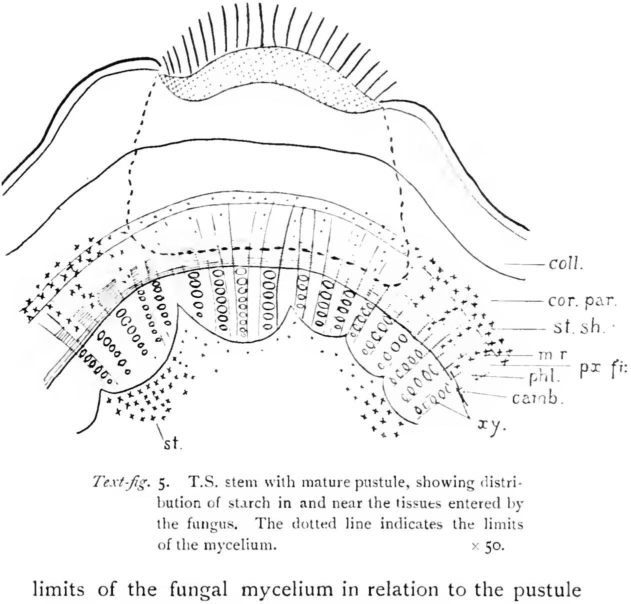
the first named states that haustoria are rarely found, I have observed haustoria, in the early stages of infection, in almost every cell of the affected region.

Text-fig. 5 shows the distribution of the starch in the affected and unaffected parts of a portion of the stem.

[1] "A contribution to our knowledge of the Morphology and Life History of *Puccinia malvacearum.*" *Phytopathology*, vol. i.

[2] "Zur sporenbildung bei Rost und Brandpilzen." *Ber. d. Deutsch. Bot. Ges.*, Bd. XXX., Ht. 8.

Abundance of starch is diagrammatically represented by the larger crosses, and less abundant starch by the smaller crosses. Normally starch is chiefly localised in the cells of the endodermis and phloem rays on the cortical side of the vascular tube and in the pith cells adjoining the inner surface of the same. This distribution is represented in the diagram to either side of the infected region. The

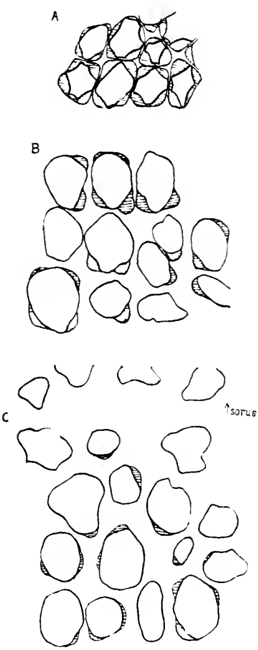


Text-fig. 5. T.S. stem with mature pustule, showing distribution of starch in and near the tissues entered by the fungus. The dotted line indicates the limits of the mycelium. × 50.

limits of the fungal mycelium in relation to the pustule are simply indicated in the figure by the dotted line. There is a marked diminution of starch in the cells of the endodermis and phloem-rays within this area. As regards these cells it is clear that the influence of the fungus is practically restricted to the area invaded. On the other hand it must be noted that the cells of the pith directly interior to the affected patch contain distinctly less starch than in the unaffected parts, although the mycelium had not actually penetrated to the pith. In the petiole the starch is similarly distributed in relation to the individual vascular bundles, and a corresponding disappearance of starch from the starch sheath and cells surrounding the inner side of the bundle was observed in affected areas.

This disappearance of starch from cells of the affected area may be due to the direct solution of the starch by the haustoria of the fungus. Such *direct* attack, however, seems improbable since the starch also disappears from the cells of the pith which are not actually reached by the mycelium. It is conceivable therefore that the fungus is *indirectly* responsible for the disappearance of the starch by tapping the carbohydrate stream directly in the phloem and thus preventing the accumulation of new reserves.

The effect of the fungus on the collenchyma is of some interest. Here the cellulose thickenings on the cell walls gradually disappear on the aggregation of the mycelium between the cells. *Text-fig.* 6A shows the normal collenchyma before any mycelium had entered the intercellular spaces, and the cellulose thickenings are represented by shading. In the case shown in *Text-fig.* 6B the mycelium, though not represented in this or the following figure, occupied the expanded intercellular spaces of a corresponding area of collenchyma. The cells showed considerable enlargement, but at this early stage the areas of thickening on the cell walls had undergone very little solution. At a later stage the cells were more widely separated by the accumulated hyphæ and the thickenings on the walls had been largely dissolved (*Text-fig.* 6C). In connection with this it may be mentioned that in both the stem and the petiole the pericyclic fibres of the vascular bundles attacked by the fungus are almost entirely devoid of thickening. A comparative study of



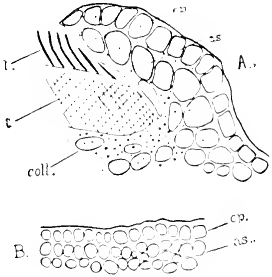
Text-fig. 6A. Collenchyma cells in unaffected area with cellulose thickening indicated.

B. Similar cells separated by mycelium of the fungus at an early stage.

C. Similar cells with mycelium between at a later stage, and the thickened areas on the walls diminished and almost completely disappeared near the teleutospore sorus. × 100.

older and younger petioles showed that the fungus does not merely prevent the formation of the thickening but actually causes its disappearance.

In addition to the enlargement of the cells of the collenchyma, already referred to, there is a very distinct increase in size of the cells of the assimilating tissue and epidermal cells overlying the young sorus (*Text-fig.* 7A), as compared with the corresponding cells from an unaffected part of the same petiole (*Text-fig.* 7B). These enlarged cells are entered by haustoria of the fungus.



Text-fig. 7A. Epidermal and assimilating cells overlying the margin of the teleutospore sorus. Mycelium between, and haustoria within most of these cells. × 200.

B. Corresponding normal epidermal and assimilating layers of petiole. × 200.

All the evidence accumulated points to the probability that the infection by the germ tube of a single sporidium is capable of producing an area of infection such as has been described. Numerous artificial infections have been made, and the course of development of the fungus from infection onwards has been studied both in seedlings and mature leaves. Kellerman[3] states that a sporidial germ

[3] *Sitz. Ber. Phys. Medic. Soc. Erlangen*, 1874.

tube penetrates the epidermis between two epidermal cells. Whilst this method of entry has been observed, I have as frequently seen the germ tube directly enter an epidermal cell as described by Rathay.[4]

The germ tube produces a slight swelling at the tip and then enters by a fine neck, expanding within the cell to form a large club-shaped infection vesicle. Whilst, in form, this resembles the haustoria to be described later, it seems better to give it a distinctive name on account of its different behaviour and functions. *Pl.* I., *Fig.* 1 shows two sporidia lying on the epidermis seen in surface view and sending their infection vesicles into the cell. For comparison a germinating sporidium, the germ tube of which has not penetrated the epidermis, is shown in *Pl.* I., *Fig.* 2. The infection vesicle, which is often directed towards the cell nucleus (*Pl. I., Fig.* 3), gives off hyphal branches (*Pl. I., Fig.* 4). These either grow through the cell wall into the neighbouring intercellular spaces, thus starting the intercellular mycelium, or one of them may enter an adjoining cell forming a second club-shaped body like the primary infection vesicle.

Sooner or later the intercellular life of the fungus is commenced and the hyphæ advance rapidly into the collenchyma and cortical parenchyma, sending short straight or forked haustoria into every cell passed (*Pl. I., Figs.* 5, 6). This rapid advance brings the mycelium very quickly into relation to the starch sheath and vascular bundle, a large number of cells being tapped in this way by means of haustoria. The mycelium now aggregates and begins to expand the intercellular spaces of the collenchyma as already described (*Text-fig.* 6). From this

[4] "Ueber das Eindringen der Sporidien-Keimschlauche der *Puccinia malvacearum* in die Epidermiszellen der *Althaea rosea*." *Verh. K. K. Zool. Bot. Ges. Wien*, 1881.

time onwards there is no great extension of the area of the host tissues invaded by the fungus, the further vegetative phase consisting in the increase in amount of the mycelium in the region already occupied.

The reproductive phase begins with the growth of fertile branches radially outwards from the mycelium massed in the collenchyma. These insinuate themselves in rows between the walls of the outer layers of collenchyma and ultimately give rise to the teleutospores, which are thus generally developed between the collenchyma and the assimilating tissue (*Text-fig.* 2). Finally the increase in size of the cells of the cortical parenchyma and of the collenchyma, together with the great accumulation of mycelium and the growth of the developing teleutospores, brings about the crushing and ultimate rupture of the assimilating tissue and the epidermis.

It is well known that the sori on the leaf often drop out after maturity leaving circular holes, and that those on the stem fall away leaving elliptical wounds.[5] These are indications that the final fate of the invaded tissues is death, and that the whole of the affected area is necrosed, none of the mycelium remaining alive in the tissues of the plant. During the earlier stages of the attack, however, it will be shown below that the affected cells remain alive.

Some confusion seems to exist in regard to the haustoria of *Puccinia malvacearum*. While, as mentioned above, Taubenhaus[6] has recently stated that haustoria are rarely found in this species, they have been described and figured by Kellerman[7] and Eriksson.[8] The descriptions

[5] Plowright, C. B., "A Monograph of the British Uredineæ and Ustilagineæ."

[6] *Loc. cit.*

[7] *Loc. cit.*

[8] "Der Malvenrost." *Kungl. Svenska. Vetensk. Hand.*, Bd. 47, No. 2. 1911.

given by these investigators do not, however, extend to the relations of the haustoria to the cytoplasm and nucleus of the cells. From many of the figures it is not even clear whether the haustoria are lying in the protoplasm of the cell or are hanging into the vacuole.

This vagueness is not by any means surprising in view of the difficulties I have encountered in dealing with fixed material of these pathological cells. It soon became evident that a fuller understanding of the delicate relationships of the haustoria to the cells could only be obtained by checking the observations on fixed material by a study of cells in the fresh or living condition. Without this comparison it was difficult to say how far the changes in the cells were due to the action of the fungus or merely to the difficulty of fixing the abnormal cells.

Haustoria are found in practically every cell of the tissues, the intercellular spaces of which are occupied by the fungus. They vary somewhat in form according to the character of the cell invaded. In the cells of the collenchyma and assimilating tissue they are generally forked, and each branch grows towards the nucleus. In the cells of the cortical parenchyma the haustoria are often larger, but when first entering appear as short straight or forked hyphæ, connected with the hypha outside by a fine neck (*Pl. I., Figs.* 5 and 6). They grow toward the nucleus, and often flatten the tip, or tips, of their branches against the nuclear membrane (*Pl. I., Fig.* 7). In the cells of the starch sheath the haustoria are generally much more branched, and the branches enter into close contact with the nucleus (*Pl. I., Figs.* 9, 10 and 11). The haustoria in the elongated cells of the phloem parenchyma enter by a narrow neck and the two branches diverge, growing in opposite directions a limited distance along the cell (*Pl. I., Figs.* 12 and 13). The nucleus of the haustorium is generally situated at the junction of the two branches and the neck. Three or more such haustoria often enter a single cell, and frequently a branch of one reaches the nucleus (*Pl. I., Fig.* 13). Haustoria approaching this form were also observed in the elongated cells of the collenchyma and in the phloem of the leaf. This difference in form is quite possibly a modification due to the elongated character of the cells entered. No haustoria were observed in the sieve tubes, though hyphæ occasionally pass into the tracheids, but do not grow for any distance along them and have not the form of haustoria. In no case were haustoria observed to grow further, but they appear to serve as definite absorbing organs of limited growth. They differ, in this respect, from the infection vesicle, branches of which grow out of the cell. In the case of the cells of the leaf, it was only found possible to study the earlier stages after entry by haustoria. *Pl. II., Fig.* 14, shows a normal cell of the spongy mesophyll of the leaf, whilst in *Pl. II., Fig.* 15, a single branched haustorium has entered such a cell, and lies in the protoplasm with the tip of one of its branches in contact with the nucleus. The nucleus is somewhat enlarged and most of its chromatin has disappeared. The chloroplasts at this stage were to all appearances perfectly normal, and this was also the case in the living material examined (*Pl. II., Fig.* 16).

In living material of the petiole examined by means of thick hand sections it is possible to observe that in the case of the uninvaded cells of the cortical parenchyma, the nucleus is generally peripherally placed in the cytoplasm which lines the cell wall and surrounds a large central vacuole (*Pl. II., Fig.* 20). A number of chloroplasts lie regularly disposed in the cytoplasm. A young haustorium entering such a cell (*Pl. II., Fig.* 21) lies in the protoplasm and grows towards the nucleus (*Pl. II., Fig.* 22), which often becomes displaced to the centre of the cell, appearing then to be slung by delicate protoplasmic strands to the peripheral cytoplasm. The haustorium enters into contact with the nucleus. Fine haustoria may also be seen to have entered and to be growing along the protoplasmic strands toward the nucleus (*Pl. I., Fig.* 7), (*Pl. II., Fig.* 17). There is at no time any appearance of an in-growing sheath or cap of cellulose as has been described for the haustoria of some other fungi.[9]

With the advance of the haustoria towards the nucleus the chloroplasts become grouped round the latter (*Pl. II., Fig.* 17). They gradually lose their contour, become paler in colour and more highly refractive, and finally lose their identity. The protoplasm near the nucleus then appears denser, and of a greenish colour on account of these disintegrated chloroplasts (*Pl. I., Fig.* 7). Ursprung,[10] in discussing the microscopic signs of death in cells, regards the wandering of the chloroplasts to the middle of the cell, the rounding of their contour and the loss of their typical structure and colour, as among the earliest signs of death. In optical section these cells, when studied fresh, have all the appearance of living cells, being quite turgid and having the cytoplasm and nucleus, which are more easily seen than in the uninvaded cells, occupying the cell as already described (*Pl. II., Fig.* 17). A series of plasmolysis experiments was tried. Thick hand sections through the living petiole bearing a pustule were mounted in water and rapidly observed, the attention being fixed on one or two of the invaded cells. These were rapidly drawn with the aid of the camera

[9] Guttenberg, "Beitrage z. physiol. Anat. der Pilzgallen," 1905, pp. 8 and 42.

[10] "Lebender Zellen um Saftsteigen." *Beihefte z. Bot. Centralb.*, Bd. XXVIII., H. 2.

lucida, and the sections were then treated with solutions of common salt. A 10% solution brought about plasmolysis of both the invaded and uninvaded cells, but on replacing the sections in water no recovery took place. When a 4% solution was used plasmolysis occurred as before, in both the invaded and uninvaded cells, but later treatment with water brought about complete recovery in both cases (*Pl. II., Figs.* 17, 18 and 19). This power of the protoplasm of the cells invaded by the fungus to contract under plasmolysis and recover again affords a definite indication that the haustoria are lying in living cells. In some cases of plasmolysis of cells invaded by haustoria, the protoplasm of the cell was observed to stream towards the nucleus along the line of the protoplasmic strands which for a few seconds became much wider. Ultimately the cytoplasm aggregated near the nucleus in the centre of the cell, though a fine protoplasmic strand still remained connected with each haustorium that had not reached the nucleus. It is difficult to see how the entering haustoria could invariably appear to be connected by protoplasmic strands with the nucleus, unless they were actually lying in the protoplasmic strand and growing along it (*Pl. II., Figs.* 22, 23). Plasmolysis experiments were also tried with salt solutions varying from 1 to 4%, in order to determine, if possible, whether there was any appreciable difference in the turgidity of invaded and uninvaded cells, but the results were inconclusive.

In the cells of all the tissues entered the haustoria appear to grow towards the nucleus. This phenomenon was first described by Rosen[11] for *Puccinia asarina*, and Magnus,[12] Guttenberg,[13] and other observers have confirmed

[11] Cohn's "Beitrage z. Biol. d. Pflanzen," Bd. VI., 1893.

[12] "Studien an der endotrophen Mycorrhiza von *Neottia nidus-avis*," *Jahrb. fur Wiss. Bot.*, Bd. XXIV.

[13] *Loc. cit.*

this for other Uredineæ. Similar observations were made by Nemec[14] on the mycorrhiza of *Calypogeia,* and by Groom[15] on that of *Thismia aseroe.* In normal cells of the Hollyhock the nucleus contains one or two nucleoli and a number of regularly arranged chromatin granules united by a network of fine threads (*Pl. II., Figs.* 20 and 24). When a cell is entered by haustoria the nucleus becomes larger and more distinctly visible in the fresh material (*Pl. I., cf. Figs.* 8 and 9), and at a late stage in some cases it shrivels somewhat (*Pl. I., Fig* 11). There is a distinct diminution of the chromatin granules, and the nucleolus becomes somewhat larger (*Pl. II., cf. Figs.* 23 and 24). Groom[16] has expressed the view that the haustoria grow towards the nucleus because it is the centre of metabolic activity, but Guttenberg[17] disputes this and holds that the former obtain specific substances, and especially chromatin and nuclear sap from the nucleus. This he believes explains the shrivelling, as well as the disappearance, of chromatin which he records. In the present example one of the most striking features is the persistence of the nucleus in an unshrivelled condition until a very late stage, so it would hardly seem likely that the haustoria withdraw the nuclear sap, nor did the appearances establish a direct utilisation of the chromatin by the fungus, though the amount of chromatin diminished.

Where an enlargement of the nucleus has been observed by previous workers, one of two conclusions have been drawn, either that the increase in size points to an increase in the metabolic activity of the cell or that it is a sign of the approaching death of the cell. In the present case, the fact that the chloroplasts aggregate

[14] *Beihefte z. Bot. Centralb.*, Bd. XVI., H. 2, 1904.

[15] *Annals of Botany*, Vol. IX., 1895.

[16] Groom, *loc. cit.*

[17] *Loc. cit.*

round the nucleus and ultimately disintegrate, and that the chromatin diminishes in amount, shows, that, though these cells are still living and plasmolysable, their vital activities are diminishing, and they are dying in a protracted fashion.

Magnus[18] records that in the case of *Puccinia leucosperma*, the haustoria arise relatively late in the attack, and therefore concludes that the main nourishment of the fungus is accomplished by the osmotic activity of the intercellular mycelium. In the present case, the fungus establishes connection with the cells of the host by means of haustoria immediately after infection. It is therefore likely that these haustoria obtain, from the living cells, materials which are essential to the development of the fungus.

The question of the relations of a parasitic fungus and the tissues of its host is a very complex one, and its exact analysis or even description is difficult. While some of the observations recorded in this paper are simply confirmatory of those recorded by other investigators, it is hoped that some advance has been made; especially as regards the exact relations of the haustoria to the cytoplasm and the nucleus of the cells attacked and the vitality of these cells. In studying this question, the simple process of examining uninjured living cells has proved of great assistance and an invaluable check on the appearances shown in permanent preparations. Another aspect of the disease caused by this fungus has been at least indicated by the study of the starch-distribu tionin the affected regions. In conclusion, I desire to express my indebtedness to Professor W. H. Lang for suggesting this work and for his continual advice and assistance during its progress. I have also had the advantage of

[18] *Loc. cit.*, p. 213.

consulting with Mr. D. Thoday on some of the physiological questions involved.

Summary.

I. The germ tube from the sporidium of *Puccinia malvacearum* penetrates the epidermis of the Hollyhock, sending an infection vesicle into the cell. This infection vesicle produces branches that grow into the intercellular spaces.

II. The mycelium grows in the intercellular spaces of the host and sends haustoria into all the cells of the affected area. Strands of hyphæ pass into the vascular bundles and there is a definite attack on the phloem region, large haustoria being sent into the cells of the phloem parenchyma.

III. In the case of the leaf each pustule is related to several of the vascular strands.

IV. There is a definite diminution in the quantity of starch in the regions invaded by the fungus.

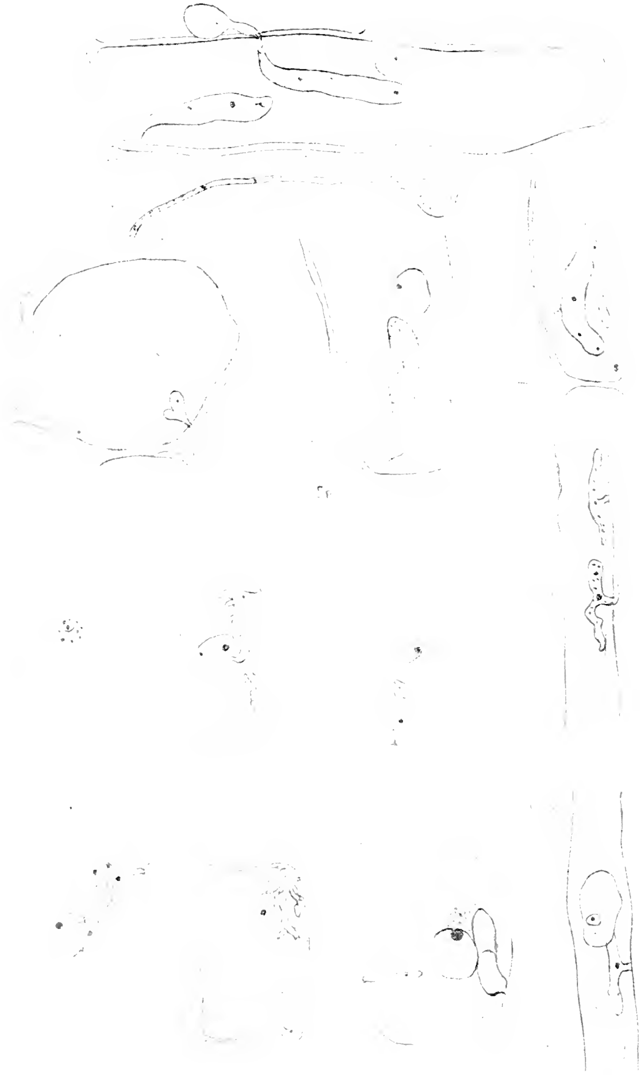
V. Cells of the host were demonstrated to remain living for a considerable time after entry by haustoria.

VI. The haustoria lie within the protoplasm and grow towards the nucleus. No case of haustoria entering the vacuole was observed.

VII. Various changes consequent on the entry of the haustoria were noted. The chloroplasts which were regularly disposed in the peripheral cyto-

plasm aggregate round the nucleus, lose their colour and contour and finally disintegrate. The nucleus moves from a peripheral position towards the centre of the cell and is connected with the peripheral protoplasm by protoplasmic strands. There is a very distinct increase in size of the nucleus and the chromatin gradually diminishes in quantity.

CRYPTOGAMIC RESEARCH LABORATORY,
UNIVERSITY OF MANCHESTER.



W. R. del.

DESCRIPTION OF FIGURES.

Plate I.

Figs. 5, 6, 17, 18, 19 and 20 were drawn from material in the fresh condition.

The remaining figures, except 1 and 2, are from sections of material fixed in Flemming's weaker solution, and stained in Carbol Fuchsin and Licht Grün.

Figs. 1 and 2 are from a strip of epidermis fixed in alcohol and stained with Erythrosin.

Fig. 1.—Two germinating sporidia lying on the epidermis seen in surface view, with their infection vesicles entering the epidermal cell. × 800.

Fig. 2.—A sporidium which has germinated, but failed to enter a host cell, showing the different character of the germ tube. × 800.

Fig. 3.—A longitudinal section of part of an epidermal cell, showing a vacuolated infection vesicle directed towards the nucleus. × 800.

Fig. 4.—A longitudinal section of a similar cell to *Fig.* 3, showing a branched infection vesicle. × 800.

Fig. 5.—A living cell of the cortical parenchyma, entered by a very young forked haustorium. The nucleus is situated in the peripheral protoplasm and the chloroplasts are normal. × 650.

Fig. 6.—A cell of the cortical parenchyma in transverse section, entered by a straight haustorium which lies in the protoplasm. × 650.

Fig. 7. A cell of the cortical parenchyma which shows an older haustorium in contact with the nucleus. The disintegrated chloroplasts lie near to the nucleus, and a young haustorium is growing along a protoplasmic strand from the opposite side. × 650.

Fig. 8.—A normal cell of the starch-sheath in longitudinal section, shows nucleus rich in chromatin granules, surrounded by large starch grains. × 800.

Fig. 9.—A similar starch-sheath cell entered by two branched haustoria, one of which is in contact with the enlarged nucleus. × 800.

Figs. 10 and 11.—Similar cells to *Fig.* 9, showing remnants of leucoplasts near to the nucleus which in *Fig.* 11 is shrivelled. × 800.

Fig. 12.—A portion of a phloem-parenchyma cell entered by a forked haustorium, with divergent branches lying in the somewhat contracted protoplasm. × 800.

Fig. 13.—A similar cell to *Fig.* 12 which shows one branch of the haustorium in contact with the hypertrophied nucleus. × 800.

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PLATE II.

Fig. 14.—A normal spongy-mesophyll cell of the leaf in transverse section. × 1,800.

Fig. 15.—A corresponding cell to *Fig.* 14 entered by a haustorium, showing one branch in contact with the enlarged nucleus. The chloroplasts are apparently normal. × 1,800.

Fig. 16.—A living palisade-parenchyma cell entered by a haustorium. The chloroplasts are apparently normal. × 900.

Fig. 17.—A living cell of the cortical parenchyma of the petiole, underlying a well-developed teleutospore sorus. Haustoria are seen lying in the protoplasmic strands and the disorganising chloroplasts are aggregated round the enlarged nucleus. Seen in optical section. × 650.

Fig. 18.—The same cell as *Fig.* 17 after partial plasmolysis with 5 % NaCl. The protoplasm contracted from the wall and was observed to cross the cell towards the nucleus. The nucleus also is somewhat contracted. × 650.

Fig. 19.—The same cell as in *Figs.* 17 and 18, after recovery from plasmolysis, in water. The protoplasm has moved back to the cell wall. × 650.

Fig. 20.—A normal living cell of the cortical parenchyma of the petiole in a corresponding position to that figured in *Figs.* 17, 18 and 19. Seen in optical section. × 650.

Fig. 21.—Similar cell (fixed) in T.S., showing peripherally-placed nucleus and a young haustorium entering from opposite side. × 650.

Fig. 22.—T.S. of a cell cortical parenchyma with a young haustorium growing towards the nucleus. × 650.

Fig. 23.—Portion of cell figured in 22, showing the connection between the haustorium and the nucleus by fine strands of protoplasm. The nucleus has lost its chromatin granules. × 1,800.

Fig. 24.—The nucleus of an unaffected cell corresponding to that seen in *Fig.* 23. × 1,800.

Manchester Memoirs, Vol. lvii. (1913), *No.* **12**.

XII. On some new Multiple Relations of the Atomic Weights of Elementary Substances; and on the Classification and Transformations of Neon and Helium.

By HENRY WILDE, D.Sc., D.C.L., F.R.S.

(Received and read July 22nd, 1913.)

In several of my papers which have been published by the Society during past years on the multiple proportions of the atomic weights, special attention was directed to the series $H7n$ on account of the magnitude and importance of its primary members in the economy of nature. Silicon (symbol Si), in combination with oxygen, constitutes more than half the weight of the earth's crust, and is the principal constituent of glass for all the purposes of civilised life. Nitrogen (N), forms nearly four-fifths of atmospheric air, and is an essential element in organic nature. Iron (Fe), from its magnetic and other physical qualities, is a necessity in modern civilisation. Gold (Au), is of æsthetic importance from the brilliancy and permanence of its colour, while the comparative rarity of its occurrence in nature admirably adapts it as a standard of commercial value.

It is not a little remarkable that while the physical properties of most of the principal elements are well determined within small fractional quantities, the atomic weight and correlated specific heat of silicon are still open to revision, notwithstanding the large amount of attention which has been given by chemists to determine these constants of nature.

In order that the connexion between the results of the present paper with my former researches may be seen without the trouble of references, I will recapitulate several of the points previously discussed, together with the explanatory tables of atomic weights.

Since the investigation of the properties of silicon by Berzelius, who regarded silicic acid as a trioxide, much discussion has arisen as to whether the atomic weight of silicon should be 21, 28, or 35, and the formulæ for its oxide Si_2O_3, SiO_2, or Si_2O_5.

Through the classical researches of Regnault the specific heat of silicon was found to be 0·176.* The determination was made with specimens of the metalloid of considerable size and in a state of compactness and purity to receive a polish which formed a perfect mirror. The above number multiplied by 28, the highest atomic weight assigned to Si, gives the product 4·93, while the law of Dulong and Petit requires the value 6·25.

In discussing the cause of the anomalous atomic heat of silicon, Regnault pointed out that in order that it might enter into the law of the specific heat of other elements, it would be necessary to write the formula of silicic acid Si_2O_5; it would then resemble that of nitric, phosphoric, and arsenic acid. The atomic weight of silicon would then be 35, and the product of this number and the specific heat would be nearly 6·25, which agrees with the analagous products that other simple bodies give. By assigning to silicon a higher atomic weight and a polybasic character like that of phosphorus or nitrogen, Regnault remarked that it is easy to explain the existence of the great number of silicates which nature presents in well-defined and beautiful crystals, and to understand the existence of the natural hydro-silicates.

* *Annales de Chemie et de Physique*, tome 53, pp. 24-31 (1861).

It will be seen from my general Table that the atomic weights of nitrogen, silicon, and iron, besides being whole numbers, are exact multiples of H7; and in all the formulæ proposed for the constitution of silica the atomic weight of silicon is a multiple of 7. These formulæ are given below, with the old and new atomic weights, the proportion of silicon to oxygen being in the ratio of 7 : 8 in all the formulæ.

1. $Si\,O = Si\,7 : O\,8 :: 7 : 8$
2. $Si\,O_2 = Si14 : O16 :: 7 : 8$
3. $Si\,O_2 = Si28 : O32 :: 7 : 8$
4. $Si\,O_3 = Si21 : O24 :: 7 : 8$
5. $Si\,O_3 = Si42 : O48 :: 7 : 8$
6. $Si_2O_5 = Si35 : O40 :: 7 : 8$
7. $Si_2O_5 = Si70 : O80 :: 7 : 8$

I have shown that the ordinal number of the typical molecule at the head of the several series in the general Table determines the quantivalence of each series of elements under it. When my first paper on atomic weights was published the only member of the series H7*n* known to be heptavalent was manganese, but I therein stated that the relation of this element of the iron group indicated a much higher quantivalence for the other members of this series than had hitherto been accorded to them. MM. Hautefeuille and Chappuis have since formed pernitric acid, which indicates a higher quantivalence for nitrogen than had previously been obtained for this element;* and more recently MM. Debray and Joly have shown that ruthenium (Ru) is heptavalent by the formation of the heptaruthenates of potassium and sodium, which have many points of resemblance to the heptamanganates.†

* *Comptes Rendus*, tome 94, pp. 1111, 1306.

† *Comptes Rendus*, tome 106, pp. 1494, 1888.

The remarkable resemblance which the members of the iron group have to each other while their atomic weights are nearly the same, has long been a subject of interest to philosophical chemists, and if the views which I have enounced respecting the formation of elementary species be correct, the cause of these resemblances admits of a possible explanation.

From the great abundance and wide distribution of iron in nature, it is probable that the vapour of this element would form an atmosphere of considerable depth; the upper and lower regions of which, by differences of pressure and temperature, might produce allotropic varieties before a definite change to the next higher members in the series occurred. When once varieties of an element were formed, these varieties would be propagated through successive condensations into the next higher members of the series, just as they are found in the palladium and platinum groups of metals. Chemists have already observed that each of the metals of the palladium group appears to be more especially correlated with some particular member of the platinum group, and all are found associated together naturally in the metallic state. M. Sergius Kern, a Russian chemist, has discovered a new metal with an approximate specific gravity of 9·39 which he classifies with the platinums, and has given to it the name of Davyum.* The low specific gravity of this element indicates it as the fourth member of the palladium group of metals, as shown in my general Table.

The chief properties which distinguish the elements of the series $H7n$ are their high fusing-point and their passivity in the presence of ordinary reagents, to which iron, under peculiar conditions, forms no exception.

Although gold in some recent classifications of ele-

* *Comptes Rendus*, tome 85, pp. 72, 623, 667.

ments has been separated from the platinum metals, yet, in its primary qualities, it exhibits closer analogies with them than with the members of any other series, and there is no other place vacant in the groups which an element with the atomic weight and physical properties of gold would fit. The constant association in nature of quartz, hematite, and specular iron ores with gold and platinum is a fact fully recognised by chemical geologists* and confirms the positions assigned for Si, Fe, and Au, in the table as forms of $H7n$.

Although I have designated the highest members of the series $H7n$ as the platinum group, yet if the small differences in their atomic weights and physical properties admit of explanation by the assumption of their being allotropic varieties of each other, then gold, palladium and iron may stand at the head of their respective groups and determine the species to which the varieties belong. It is no objection to the theory of the members of the respective groups being varieties of each other that they cannot by any known power of analysis be resolved into their primaries, as the same objection would apply to the natural varieties of organic species determined by naturalists.

The arbitrary policy of several writers in doubling the atomic weights of four of the gaseous members of the series $H7n$, viz.: neon, argon, krypton and xenon, (notwithstanding that the atomic weights of nitrogen, hydrogen, oxygen and chlorine are the same as their specific

* Bischoff's Chemical and Physical Geology, Vol. 3, p. 534.
Cavendish Soc. Works. Murchison's Siluria, Chap. 17, pp. 433-439.

gravities at ordinary temperatures) has induced me to review the multiple relations of the series of elements H$7n$ with the following results:—

1.	Ne 7 + Ar	21 =	28 ÷ 2 =	N	14
2.	Ne 7 + Si	35 =	42 ÷ 2 =	Ar	21
3.	Ar 21 + Xe	63 =	84 ÷ 2 =	Kr	42
4.	N 14 + Fe	56 =	70 ÷ 2 =	Si	35
5.	Ne 7 + Pd	105 =	112 ÷ 2 =	Fe	56
6	N 14 + Au	196 =	210 ÷ 2 =	Pd	105
7.	Ne 7 × 28	...	... ... =	Au	196
8.	Ne 7 × 9	...	... ... =	Xe	63

An examination of the above Table shows (1) that no less than six triads are formed in the series, in which the sum of the atomic weights of the extreme members is double the atomic weight of the means, and are all multiples of Ne 7. Triads of atomic weights have been fully recognized by Dumas, Faraday, and other philosophical chemists as indubitable evidence of community of origin, of transmutation, and important factors in the classification of elementary substances. (2) That the atomic weight of silicon (35) follows naturally in the series, and the lesser values Si 21, and Si 28, find no place in the table. (3) That if the atomic weights of Ne, Ar, Kr, Xe, were doubled, it would be necessary to double those of N, Si, Fe, Pd, Au, and other members of the series, which is obviously absurd.

As the four new elementary gases have no chemical properties their specific gravities are necessarily substituted for atomic weights. The experimental determination of their respective densities differ slightly from my table owing to the difficulty of separation from each other, probably from other causes which it is unnecessary on this occasion to indicate. In the interests of science, however, it is incumbent on me to say that no teacher of natural science, with the above table before him, is at liberty to double the atomic weights of the inert gases of the series H7*n* without violence to his moral intelligence and lasting injury to the ingenuous student who looks up to him for guidance and instruction. These remarks are equally applicable to the doubling of the atomic weight of helium, which element has been separated from the series H2*n*, and grouped with Ne, Ar, Kr, Xe, solely on account of its chemical inertness, the five elements having no other rational classification.

It is a common error to assume that discoverers in various departments of science are, necessarily, authorities on the co-ordination of the subject of their discoveries with the general properties of bodies, and with the real nature of things. Thus (1) Peligot adopted 120 as the atomic weight of uranium, and Stromeyer 56 for cadmium, the modern determinations for these elements being 240 and 112 respectively. (2) Scheele's oxymuriatic acid was shown by Davy to be elementary chlorine. (3) Platinum was identified by its Brazilian discoverer with silver, and derived its name from that metal. Many similar instances may be adduced from other departments of the natural sciences. It will be sufficient to mention in this connexion the discovery and first appearance of Saturn's rings, the supposed cometary nature of the planet Uranus, and the landfall of Columbus.

Helium, as will be seen in several of my papers, is the typical element of the series H2*n*, with an atomic weight of 2 (He = 2) as shown in the following table. This number has been adopted by French chemists in the table of atomic weights published in the *Annuaire du Bureau des Longitudes*.

Hn	Sp. gr.	Acc. wt.	Diff.	H2n	Sp. gr.	Acc. wt.	Diff.
H = **1**			Diff.	He = **2**			Diff.
			− 6				− 6
0 . 0 . 7 Li = **7**	0·59†	7*		0 . 0 . 8 = Be = **8**	1·64	9·2	
			− 16				− 16
1 × 23 . 0 = Na = **23**	0·98	23		1 × 24 − 0 = Mg = **24**	1·74	24	
			− 16				− 16
2 × 23 − 7 = K = **39**	0·86	39		2 × 24 − 8 = Ca = **40**	1·8	40	
			− 23				− 24
3 × 23 − 7 = Cu = **62**	8·9	63·3		3 × 24 − 8 = Zn = **64**	7·2	65	
			− 23				− 24
4 × 23 − 7 = Rb = **85**	1·52	85		4 × 24 − 8 = Sr = **88**	2·54	87·5	
			− 23				− 24
5 × 23 − 7 = Ag = **108**	10·6	108		5 × 24 − 8 = Cd = **112**	8·69	112	
			− 23				− 24
6 × 23 − 7 = Cs = **131**	1·88	132		6 × 24 − 8 = Ba = **136**	3·75	137	
			− 23				− 24
7 × 23 − 7 = X = **154**	12·2‡			7 × 24 − 8 = X = **160**	10·13‡		
			− 23				− 24
8 × 23 − 7 = X = **177**	2·2‡			8 × 24 − 8 = Ra = **184**	4·8‡		
			− 23				− 24
9 × 23 − 7 = Hg = **200**	13·6	200		9 × 24 − 8 = Pb = **208**	11·44	207	

Further inspection of the table will show that, in consequence of the law of multiple proportions by which the atomic weights of the series are determined, there is a

* Accepted Atomic Weights. † Specific Gravities. ‡ Estimated.

common difference of 23 between the atomic weights of the series H*n* (commencing with K) to the final member Hg. In like manner there is a common difference of 24 in the strictly parallel series H2*n*. The regular parallel differences between the atomic weights of members at the head of both series are equally remarkable.

I have discussed in former papers the alternation of light and heavy metals in regular order observable in each of these series, and have put forward suggestions as to its possible cause in my first papers on the "Origin of Elementary Substances," published by the Society in 1878 and 1887.

Radium (as was indicated in my Table of Elements some years previous to its discovery), is one of the synthetic transformations of helium in the series H2*n*, and is the next higher member to barium of the alkaline-earth metals. This place has since been assigned to radium by Mme. Curie, but with an erroneous atomic weight and specific gravity ; as will be seen by comparison with the similar properties of the other members of the same series.

Helium (H2) is also shown in the paper of 1878* as the analytic transformation ultimate of radium and other members of the series H2*n*.

The positions of helium and neon in my general Table of Elements, as the transformation ultimates of the series H2*n* and H7*n* respectively, are further interesting in connexion with the recent announcements that these elements have been found in glass vessels in which they had no previous existence.† Now, assuming the reality of these

* *Proc. Manchester Literary and Philosophical Society*, vol. xvii, p. 194, 1878 ; *Memoirs*, vol. xxx, 1887. *Chem. News*, vol. 38, 1878.

† *Nature*, February 13th. *Chem. News*, February 14th, 1913.

observations, the phenomena not only admit of explication from my classification of the elements, but also account for the discordant results obtained by the experimenters engaged in the research.

Several significant facts were brought out by the investigators during their researches, viz.:—the use of old X-ray tubes, bare glass tubes, and glass wool, all from which neon and helium were obtained. One of the investigators could only find neon as a transformation product, while others, working independently, found helium alone, and in other cases a mixture of both gases.

These results were of sufficient interest to induce me to ascertain the composition of various glasses used in the arts, from which the following are selections:—*

1.	Baryta flint glass	Si, Ba, K.
2.	Ordinary flint glass ...	Si, Pb, K.
3.	Plate and Window glass	Si, Ca, Na.
4.	X-ray glass	Si, B, Na.

All the above glasses, as will be seen, have silicon (Si) as their principal constituent, the transformation ultimate of which is neon.

The next constituents of the first three glasses, barium, (Ba), lead (Pb), and calcium (Ca), are members of the series $H2n$; the transformation ultimate of which is helium.

The alkali metals, sodium (Na) and potassium (K), as will be seen, are constituents of nearly all glasses, and their transformation ultimate, with other members of the series Hn, is hydrogen. Considering the wide diffusion of this element, the transformations of Hn when actually effected would be difficult to demonstrate. One of the two principal lines in the spectrum of thallium was masked

* Thorpe's Dict. of Applied Chemistry, Article—Glass.

completely by hydrogen C of atmospheric air for more than thirty years, when I discovered it in the arc spectrum while engaged in another research.*

As the transformation of radium into helium was first effected, indirectly, through its hydrated halogen and oxygen combinations in aqueous solutions, so would the anhydrous oxides of other members of the series $H2n$ (and of other series) be resolved into their ultimates through the medium of their silicates, acting as solutions, during the process of transmutation.

All the silicates of the series $H2n$, and some of other series, are easily vitrified in small quantities in laboratory crucibles and appliances. Their spectra can then be examined during electrification in tubes (under suitable conditions of temperature and pressure) for the discovery of new elements and the identification of those already known.

It may be laid down as a canon of chemical science, that the members of any natural series of elements are never transformed into the members and ultimates of any other series. Thus neon is the permanent transformation ultimate of the series $H7n$; just as helium is the fixed ultimate of the series $H2n$. Hence, also, helium and neon will be evolved from the vitrified silicates of calcium and barium. By the substitution of sodium and potassium for calcium and barium in their vitrified silicates, the transformation products would be neon and hydrogen, but without helium.

* *Proc. Roy. Soc.*, 1893.

TABLE I

Elementary Substances, with their Atomic Weights in Multiple Proportions, [illegible] 1801 [illegible]

Series	+ Hn		+ H2		H3	H4	H5	H6	H7
1	H = 1		He = 2						Ne 7
2	Li = 7 7* 0.59†		Be 8 9.2 1.64		C = 12 12 1.71	16	B 10 11 2.63	18	N 14 Ar 21 Kr 42 Xe 63
3	Na = 23 23 0.98	F = 19 19	Mg 24 24 1.74	O = 16 16	Al 27 27 2.56	32	P 30 31 1.82	— 36	Si 35 2.49
4	K = 39 39 0.86	Cl = 35 35.5 1.3	Ca = 40 40 1.8	S = 32 32 2.05	Sc 42 44 3.4	Ti 48 48 4.1	V 50 51 5.5	Cr 54 52.1 7.3	Fe 56 56 [illegible] Mn 56 55 Ni 56 58 [illegible] Co 56 59 [illegible]
5	Cu = 62 63.3 8.9		Zn = 64 65 7.2		Ce 69 92 141 6.5	Ge 72 72.7 5.47	As 75 75 5.63		
6	Rb = 85 85 1.52	Br = 81 80 3.0	Sr 88 87.5 2.54	Se 80 79.4 4.8	Ga 96 70 5.95	Zr = 92 90 4.15 5.4	Nb 95 94 6.67	Mo 96 96 8.6	
7	Ag 108 108 10.6		Cd 112 112 8.69		Y 123 91.7 89.5 8.1‡	Sn 116 116 7.29	Sb 120 12[illegible] 6.72		Pd 105 [illegible] Rh 105 [illegible] Ru 105 [illegible] Da 105
8	Cs = 131 132 1.88	I = 127 127 4.95	Ba 136 137 3.75	Te = 128 128 6.3	In 150 75 113.4 7.42	La 140 139 6.7	X 140 5.15	X 144 [illegible]	
9	X = 154 12.2‡		X 160 10.13‡		Er = 177 170.6 9.4‡	X 164 9.11‡	X = 165 8.30		
10	X = 177 2.2‡		Ra 184 4.8‡		Tl 204 204 11.85	D = 188 95 8.0‡	Ta 185 182 10.75 [illegible]	W 186 184 18.26	
11	Hg = 200 200 13.6		Pb = 208 207 11.44		Th 231 231.4 11.23	U = 240 240 18.4	Bi 210 210 9.83		Au 196 196 19.3[illegible] Pt 196 197 21.5[illegible] Ir 196 19[illegible] 22.42 Os 196 198 22.48

*Accepted Atomic Weights †Specific Gravities ‡Estimated §[illegible] ‖[illegible]

*** The accepted atomic weights are taken from the standard works and tables of Wurtz, Roscoe and Schorlemmer, [illegible] Cooke, [illegible] Clarke and Watts' "Dict. Chem.," Supp., p. 247—[illegible]

XIII. On the Influence of Atmospheric Pressure, Temperature and Humidity on Animal Metabolism.

By William Thomson, F.R.S.E., F.I.C., F.C.S.

(*Read March 21st, 1911. Received for publication, April 22nd, 1913.*)

In a previous paper[1] I pointed out that in making determinations of the percentages of carbon dioxide (CO_2) contained in the air exhaled from the lungs of different persons, that on certain days the percentages of CO_2 were all, or nearly all, relatively high whilst on other days they were all, or nearly all, relatively low.

That a number of persons should be thus similarly affected gave presumptive evidence that the cause lay, not in the individuals, but in the conditions of the atmosphere.

I therefore commenced a series of carbon dioxide (CO_2) determinations in the exhaled air from a number of persons, making at the same time observations of the barometer, the hygrometer and the thermometer.

I had previously determined that the percentage of CO_2 in the exhaled breath was greater when the air inhaled had been previously dried by passing over strong sulphuric acid, and I assumed therefore that it would be found that the metabolism would be greater when breathing dry than when breathing damp air.

Determinations made showed greater percentages of CO_2 in the exhaled air at high elevations than in the valley, and higher in the valley than at the bottom of a deep coal pit, and this led to the assumption that a fall of the barometer would produce an increase in the percentage of CO_2 (increased metabolism) whilst a rise in the barometer

[1] Read before the Seventh International Congress of Applied Chemistry, 1909, London. Section VIII. A., Volume Hygiene and Medicinal Chemistry, p. 154.

would produce a decrease in the percentage of CO_2 (decreased metabolism).

Determinations made when the body was surrounded by cold air showed greater metabolism than when it was surrounded by warm or hot air, such comparisons having been made in air at the ordinary temperatures as compared with the air of a Turkish bath. Other observations, however, shewed that whilst the body remained in cold air the metabolism was greatly increased whilst breathing warm or hot air.

Incidentally I may mention that the breathing of pure dry oxygen did not produce greater metabolism than the breathing of ordinary dry town's air, although the oxygen was in such excess that a red hot tip on a splinter of wood burst into flame when put into a jar filled with the exhaled air, and finally that highly ozonised air produced a decrease in the metabolism.

In considering the effects of the altered conditions of the atmosphere on animal metabolism not only must the counteracting influences of pressure and humidity be considered but also that of temperature and the condition of the person breathing, because we found that the metabolism was much increased for some time after taking violent exercise.

Finally the mode of breathing had to be carefully taken into account, the percentage of CO_2 being increased by retaining the inspired air longer than usual in the lungs.

The first experiments were made by breathing three times through a glass tube $\frac{3}{8}$-inch diameter into an 8-ounce bottle provided with an accurately-fitting well-vaselined glass stopper, previously expelling about a third of the breath to get rid of the residual air from the mouth and throat which had not been in contact with the lungs, inserting the stopper when the tube was gradually withdrawn without stopping the exhalation. After removing the stopper under a concentrated solution of brine which had been saturated with carbon dioxide at the ordinary temperatures and pressures, a rubber cork provided with two glass tubes was then introduced into the mouth of the bottle under the liquid in the brine trough, the air displaced into a 100 c.c. gas burette and the CO_2 absorbed by transferring the air into an absorption tube containing caustic potash (which surrounded the measuring tube to take up less space in the box). It was found, however, that although the results so obtained were as a whole relatively good, this method of collecting the exhaled air tended to make the individual conscious that he was breathing, and consequently the breathing tended to become abnormal.

The final method adopted for collecting the exhaled air was by breathing into a metallic gas-holder dipping into brine, similar to that used in gas works, the gas-holder being counterpoised by weights. This device made it more easy to breathe naturally into the apparatus as the whole of the residual air from the mouth and throat was also introduced, and nothing required attention except the tap which was opened or closed as desired. 100 c.c. of the mixed air from the gas-holder, after throwing away the first portion to clear the tubes, was taken into the gas burette and analysed as above described.

HUMIDITY.

The humidity of the air was determined by the wet and dry bulb thermometers having a scale of 20 degrees to the inch. I do not consider these thermometers satisfactory. Subsequent experiments, in which very delicate thermometers were used having a scale of 20 degrees to $7\frac{1}{2}$ inches, gave wonderfully accurate results when compared with determinations made by passing measured volumes of air through a Stromeyer coil absorption tube containing strong sulphuric acid and weighing the moisture so absorbed.

The humidity determinations can therefore only be regarded as roughly and approximately correct.

The following figures and the diagrams shew the results obtained from the exhaled air from four persons breathing town air.

It was found that under normal conditions an average greater metabolism takes place in one person than another. Thus the following persons are those whose exhaled air was analysed in the series of experiments to be put before you, and the following figures shew the ranges of CO_2 contained in them :—

			CO_2 contained in exhaled air in Town. Per cent.
Dr. W., age	38	... ...	3·7 to 4·2
J. W.	„ 22	... ...	5·4 to 6·2
N. T. F.	„ 21	... .	3·6 to 4·3
B. S.	„ 15	... .	4·2 to 5·3

These figures were obtained in my Laboratory in Manchester. It was found, however, in previous experiments that exhaled air containing about 4 per cent. in town rose to about 5 per cent. of CO_2 when breathing uncontaminated air in the country, shewing an increase of about 25 per cent. when the samples were taken in bottles as above described.

In the final series of experiments *(see Table at end)* the determinations were made by the gas-holder arrangement between 9 and 10 in the morning and between 2 and 3 in the afternoon of each day. It was found that in the afternoon the average results were somewhat higher than in the morning.

GUINEA PIG AND MOUSE EXPERIMENTS.

With a view to finding whether the same relative results would be obtained from the exhaled air of guinea pigs and mice I endeavoured to find the method for obtaining the most satisfactory results.

The first experiments were made by putting the animal under a bell jar and drawing air from it through strong sulphuric acid and then over soda lime to absorb the CO_2, but I found by the appliances I used, suitable for delicate weighing of the soda lime tubes, that I could not pass sufficient air through the bell jar to prevent a high contamination of the air breathed by the animal, and so the following was devised which enabled the animal to breathe comparatively pure air during the progress of the experiment.

A specially accurate gas meter, previously tested, capable of passing 12 cubic feet per hour, was connected on the one side to a vacuum pump and on the other to a large bottle to minimise the slight vibrations from the water pump. This was connected by a glass tube with two bottles, each of 1 litre capacity, for receiving the breathed air for analysis.

The ordinary air was drawn into the bell jar from outside the laboratory with glass tubes entering at the top and leaving at the bottom of the bell jar, after passing through a bottle of one litre capacity, in which the CO_2 in the air used was determined and deducted from that obtained after passing over the animal, the animal being supported above the perforated glass plate on which the bell jar stood by means of a piece of iron wire gauze.

The air from the bell jar passed along a tube which ended in a T-piece, the junction of which was provided with a two-way tap, by which means the stream of air could be diverted to the right or to the left at will.

Tubes were connected to the right and to the left tube of the T-piece respectively; these were attached to the longer of two straight glass tubes passing through rubber stoppers, one going to the bottom of the bottle, the other short glass tube only passing through the rubber cork, and these were connected with a corresponding but reversed T-piece and two way glass tap, and the stem of this T-piece was connected to the large bottle communicating with the meter and the water vacuum pump: thus, by turning on the water to the vacuum pump, air was drawn through the bell jar, and, through one or the other of the litre bottles standing side by side according to the direction in which both of the two-way taps were turned, and by reversing both of these taps the stream of air could be completely shut off from one bottle and passed through the other. This enabled me to take samples of the air without interfering with the continuity of the experiment.

The carbon dioxide in the air in the litre bottles was determined by running in through the straight smaller glass tube which penetrated the stopper, 15 c.c. of $\frac{1}{10}$ N. baryta water solution, shaking vigorously and then titrating with $\frac{1}{100}$ N. hydrochloric acid solution.

The current of air passing through the bell jar was increased till the air in the bottles contained about 20 parts by volume of CO_2 per 10,000 of air. It was found necessary to pass air at the rate of about 80 to 100 litres equal to 2·8 to 3·2 cubic feet per half hour to give this result, the current being passed alternately through the two bottles till the CO_2 found was about constant.

The total weight of CO_2 in the air used per hour was then calculated, and this was further calculated into grammes of CO_2 per 1,000 grammes weight of animal, which was always weighed immediately before putting it under the bell jar.

Only a small number of experiments were recorded for the mice. It was found that they excreted besides acetamide certain amines or ammoniated compounds, which neutralised and fixed some of the carbon dioxide, and thus showed by the baryta water test a smaller proportion of carbon dioxide than it should have done.

The final results were obtained by putting the mouse on the top of several layers of blotting paper, previously saturated with a strong solution of citric acid, and dried, which fixed the amines and ammoniacal compounds and prevented them from interfering with the CO_2 results.

The figures obtained from the guinea pig experiments are shewn in the tables and charts appended hereto.

From the results given it will be seen that, on all occasions where the barometer, hygrometer or thermometer alters appreciably, there is a corresponding change in the percentage of CO_2 in the exhaled air of *all*, or nearly all, the persons or animals tested. A rise in the barometer producing a fall in the CO_2, and a fall in the barometer producing a rise in the CO_2 exhaled, and a marked increase in humidity producing a fall, and a decrease producing a rise in the CO_2 exhaled.

The rise of the temperature of the air produced a lowering of the CO_2 in the exhaled air, and a fall in the temperature produced a rise. The rate of the pulse seems to have no influence on the percentage of CO_2 in the exhaled air.

The temperature (sub-lingual) of the body appeared also to have little influence on the CO_2 in the expired air.

I have pleasure in acknowledging the services of my assistant, Mr. Norman T. Fox, who has carried out these series of experiments with great care and ability.

CO_2 IN EXHALED AIR FROM MICE.

Date.	Barometer. mm.	Hygrometer. Dry bulb. Fah.	Hygrometer. Wet bulb Fah.	Hygrometer. Percentage of humidity 100 = saturation.	Hygrometer. Difference between wet and dry bulb thermometers.	Experiment commenced.	Weight of mouse in grammes.	Vol. of air per ½ hour used at N. T. P. c.c.	Grms. CO_2 per 1,000 grms. weight of animal per hour.
1910.						P.M.			
July 1 ..	749·3	63	60	82	3	3-0	12	50,000	8·6321
,, 2 ..	746·7	60·5	58	85	2·5	3-10	10	51,222	8·7490
,, 4	750·9	60	57	82	3	2-45	13	49,394	8·7020
,, 5 . .	758·1	60	57	82	3	3-30	14	48,263	8·7000
,, 6 . .	750·5	60·5	59	91	1·5	3-0	11	48,364	8·9010
,, 7	756·9	61	59	88	2	3-0	12	50,230	8·8993

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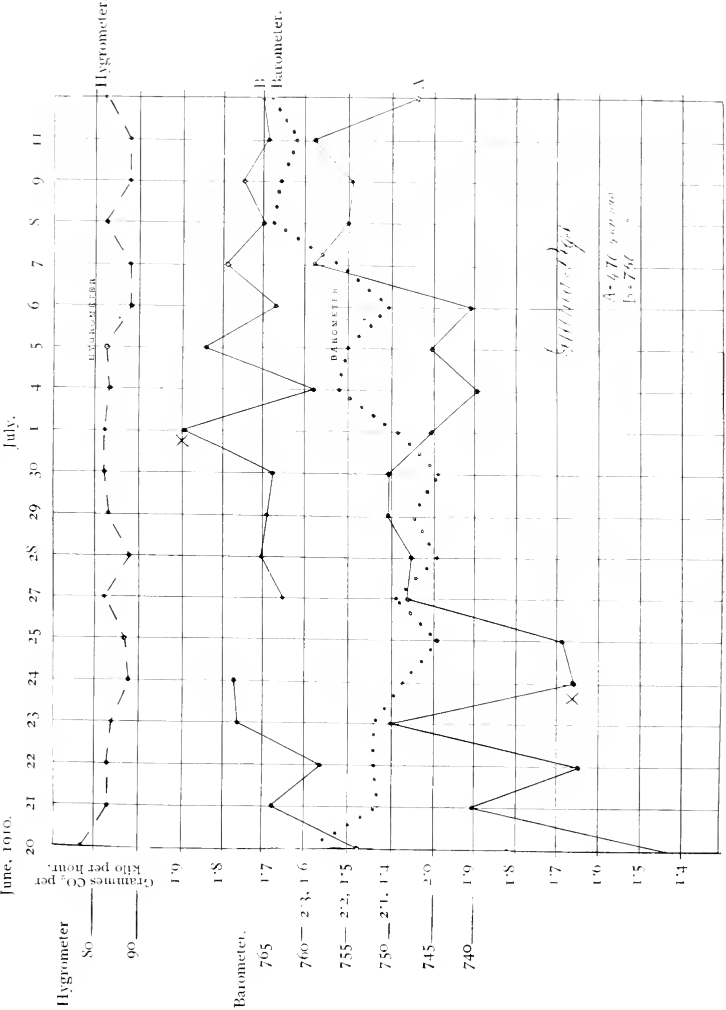
GUINEA PIG EXPERIMENTS.

GUINEA PIG EXPERIMENTS.

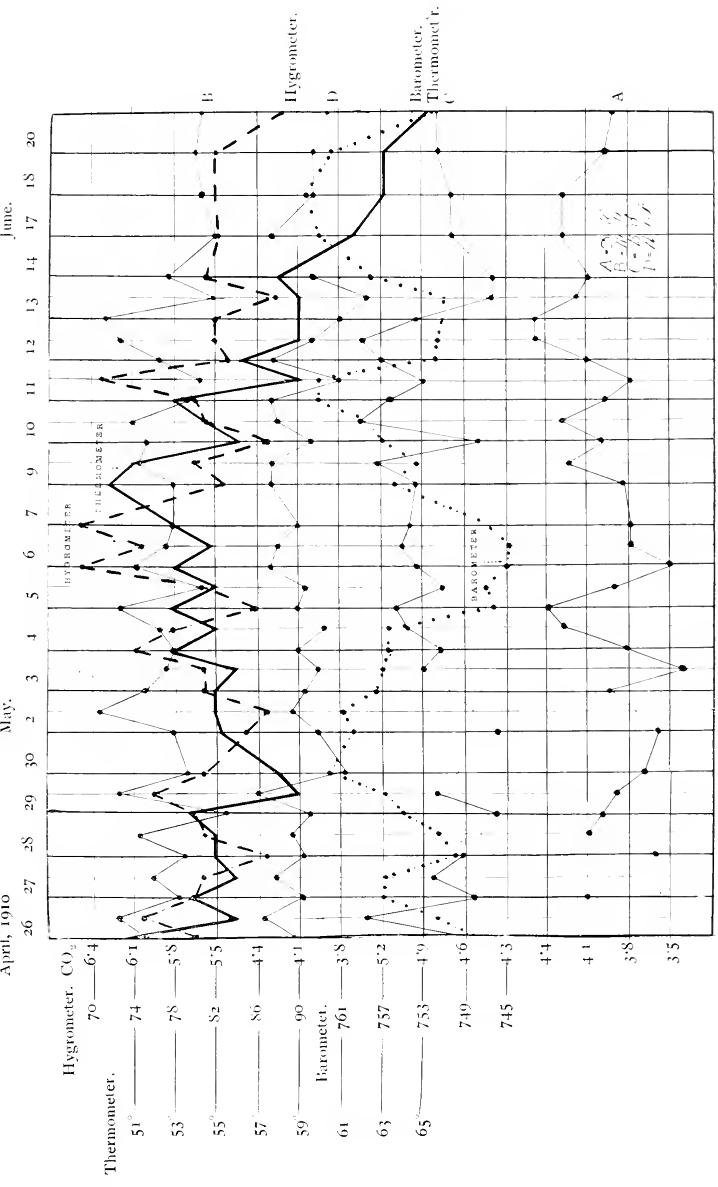
GUINEA PIG EXPERIMENTS

Month	Day	Barometer mm. Mercury	Barometer Difference mm.	Hygrometer F. Wet bulb	Hygrometer F. Dry bulb	Percentages of Humidity 100 = Saturation	Volume of air used per 1 hour, calculated to 0° and 760	Grammes CO₂ per kilo animal per hour Animal A	Grammes CO₂ per kilo animal per hour Animal B	Remarks
June	7	755·6		67	69	88	85,811	1·658		
	8	754·3	− 1·3	61	63	88	94,884	1·777		
	9	751·8	2·5	62	64	88	87,618	1·629		
	11	751·8	0	62	64	88	84,638	1·686		
	13	754·3	+ 2·5	61	64	82	84,780	1·669		
	16	763·2	+ 8·9	60	63	82	76,080	1·736		
	17	763·2	0	59	64	72	85,194	2·246		Animal apparently unwell
	18	764·5	+ 1·3	60	63	82	87,612	2·287		
	20	762·0	− 2·5	60	64	77	81,501	1·428	1·468	
	21	756·9	− 5·1	63	66	83	87,230	1·894	1·677	
	22	752·0	− 4·9	62	65	83	83,946	1·639	1·557	
	23	753·1	+ 1·1	61·5	65	80	87,292	2·092	1·762	Animal "A" unwell
	24	748·0	− 5·1	62	64	88	88,474	1·659	1·772	
	25	744·7	− 3·3	61·5	64	85	87,322	1·670		
	27	749·3	+ 4·6	58·5	61	85	93,576	2·0529	1·6570	Animal "A" unwell from 27th June till 12th July
	28	744·7	4·6	61	63·5	85	89,772	2·047	1·703	
	29	746·7	+ 2	61	64	82	88,320	2·103	1·692	
	30	744·2	2·5	60	63	82	88,102	2·102	1·583	
July	1	749·3	5·1	60	63	82	88,200	2·003	1·893	Animal "B" very restless under bell jar.
	4	756·9	+ 7·6	57	60	82	80,004	1·887	1·583	
	5	756·5	− ·4	57	60	82	88,645	1·994	1·843	
	6	750·9	− 5·6	59	61	90	87,040	2·280	1·671	
	7	759·4	+ 8·5	59	61	90	90,900	2·199	1·796	
	8	759·4	0	58	61	82	91,900	2·193	1·706	
	9	758·5	− ·9	59	61·5	85	90,100	2·288	1·753	
	11	756·9	− 1·6	59	61·5	85	87,528	2·031	1·691	
	12	759·4	+ 2·5	59	62·5	80	88,580	2·002	1·703	
August	13	760·0	+ ·6	63	65	88	86,640	1·570		

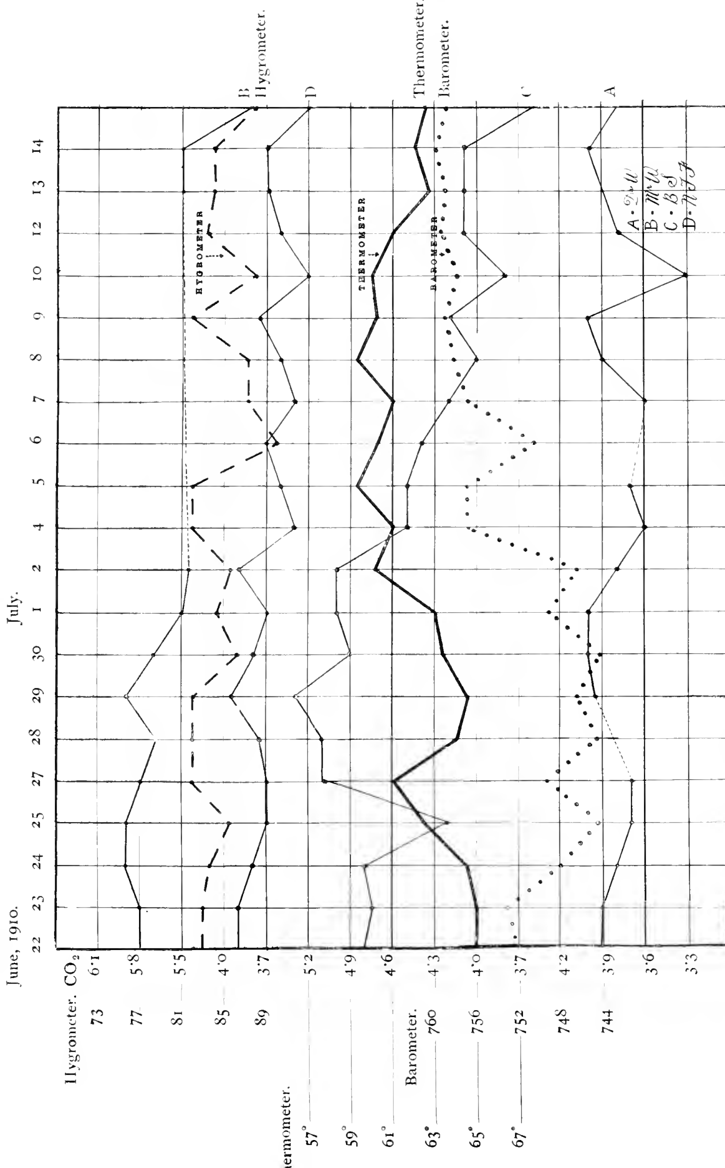
Animal "A" apparently unwell



GUINEA PIG EXPERIMENTS.



CO_2 IN EXHALED AIR EXPERIMENTS FROM 4 PERSONS.



CO_2 IN EXHALED AIR EXPERIMENTS FROM 4 PERSONS.

,, 21...	752·6	−9·4	65	63	6·0	88	+6	78	—	4·0	93	—	5·0	97	—	4·8	80	—	3·9
,, 22...	752·0	−0·6	65	62	5·6	83	−5	78	—	3·9	97	—	5·8	79	—	4·8	72	—	3·9
,, 23...	753·1	+1·1	65	62	5·6	83	0	80	—	3·9	90	—	5·8	80	—	4·8	66	—	3·9
,, 24...	748·0	−5·1	64½	62	5·5	85	+2	84	—	3·8	103	—	5·9	83	—	4·8	70	—	3·8
,, 25...	744·7	−3·3	63½	61	5·5	85	0	92	—	3·7	101	—	5·9	75	—	4·2	75	—	3·7
,, 27...	749·3	+4·6	61	58½	5·1	85	0	82	—	3·7	92	—	5·8	99	—	5·1	75	—	3·7
,, 28...	744·7	−4·6	63½	61	5·5	85	0	—	—		93	—	5·7	82	—	5·1	82	—	3·70
,, 29...	746·7	+2·0	64	61	5·4	82	−3	84	—	3·90	96	—	5·9	83	—	5·3	69	—	3·95
,, 30...	744·2	−2·5	61½	59	5·1	85	+3	82	—	4·0	91	—	5·7	80	—	4·9	73	—	3·8
July 1...	749·3	+5·1	63	60	5·2	82	−3	86	—	4·0	94	—	5·5	83	—	5·0	82	—	3·7
,, 2...	746·7	−2·6	60½	58	5·0	85	+3	84	—	3·8	98	—	5·40		—	5·0	60	—	3·9
,, 4...	756·9	+10·2	60	57	4·7	82	−3	80	—	3·6	—	—	—	—	—	4·5	63	—	3·5
,, 5...	756·9	0	60	57	4·7	82	0	84	—	3·7	—	—	—	—	—	4·5	—	—	3·6
,, 6...	750·5	−6·4	60½	59	5·3	91	+9	—	—	—	—	—	—	—	—	4·4	—	—	3·7
,, 7...	756·9	+6·4	61	59	5·2	88	−3	76	—	3·6	—	—	—	—	—	4·2	—	—	3·5

DATE	BAROMETER		HYGROMETER					[illegible]			W			B. S.			[illegible]		
1910	mm	mm rise or fall	Dry Fah	Wet Fah	Grns per c. ft.	Percentage of Humidity to Saturation	Rise or fall percentage Saturation	Pulse	Temp	CO_2	Pulse	Temp	CO_2	Pulse	Temp	CO_2	Pulse	Temp	CO_2
April 26	748		51	48	3·4	80	—	—	—		80	97·0	6·0	77	97·0	4·0	89	97·0	4·1
,, 26	751	·3	56	52	3·8	75	−5		—	4·3	93	97·2	6·2	82	97·2	5·3	70	97·2	4·3
,, 27	750·0	5·0	54	51	3·8	80	+5		—	4·1	87	97·4	5·7	83	97·4	4·5	68	97·0	4·0
,, 27	750·6	0·3	56	53	4·1	81	+1				90	97·5	5·0	71	97·0	4·8	70	97·2	4·2
,, 28	749·3	7·3	55	53	4·2	87	+6	—	—	3·6	86	98·0	5·7	81	97·4	4·6	86	97·5	4·0
,, 28	751·3	2	55	52	3·9	81	6	72	96·8	4·1	95	98·5	6·0				71	97·7	4·1
,, 29	754·[illegible]	3·5	54	51	3·8	80	1	64	97·2	4·0	80	97·8	5·4	70	97·3	4·3	83	98·0	4·0
,, 29	750·0	2·1	59	55	4·3	76	4	65	97·5	3·9	74	98·6	6·2	72	98·1	4·8	73	98·0	4·4
,, 30	762·0	5·1	58	55	4·4	81	5	76	97·3	3·7	87	98·0	5·7				78	97·0	3·7
May 2	759·4	2·6	55½	54	4·4	[illegible]	[illegible]				80	98·0	5·8				63	97·5	3·9
,, 2	760·0	1·5	55	53	4·2	87	3	72	98·0	3·6	[illegible]	98·5	6·[illegible]	88	98·0	4·9	72	97·5	4·1
,, 3	757·4	3·5	55	52	[illegible]	81	6	74	98·0	3·90	91	98·3	6·0				83	98·0	4·0
,, 3	750·[illegible]	0·5	56	53	4·1	81	0	78	97·8	[illegible]	90	98·7	5				78	97·3	3·9
,, 4	75[illegible]·4	0·5	53	49	3·4	74	7	75	97·0	3·8	[illegible]	98·[illegible]	5·5	[illegible]	97·0	4·70	75	97·7	4·1
,, 4	75[illegible]	[illegible]	55	52	[illegible]	74	0	7[illegible]	97·[illegible]	[illegible]	[illegible]	[illegible]·7	5·[illegible]	7[illegible]	[illegible]	[illegible]	71	97·8	[illegible]
,, 5	746·2	1·2	53	5[illegible]	3·9	80	2	70	97·4	4·[illegible]	[illegible]	97·8	6·2	70	[illegible]	5·1	[illegible]	98·2	4·1
,, 5	747·0	0·8	55	52	3·9	81	5	70	[illegible]	3·9	[illegible]	98·4	5·[illegible]		[illegible]	4·7	74	97·9	[illegible]
,, 6	744·0	2·[illegible]	5[illegible]	48	3·1	[illegible]	12	7[illegible]	97·0	3·3	91	[illegible]	6·1	74	[illegible]	4·0	[illegible]	98·[illegible]	4·3
,, 6	734·0	[illegible]	55	51	3·7	75	6	7[illegible]	97·4	3·[illegible]	[illegible]	98·6	[illegible]	[illegible]	[illegible]	[illegible]	74	98·0	4·20
,, 7	74[illegible]·7	[illegible]	[illegible]	[illegible]	3·1	[illegible]	6	72	[illegible]	3·8	[illegible]	[illegible]	5·[illegible]	[illegible]	[illegible]	5·[illegible]	76	97·8	4·1
,, 9	750·8	[illegible]	5[illegible]	47	3·[illegible]	[illegible]	14	70	97·6	3·[illegible]	[illegible]	98·2	[illegible]	7[illegible]	[illegible]	[illegible]	[illegible]	97·7	4·[illegible]
,, 9	753·[illegible]	[illegible]	5[illegible]	[illegible]	[illegible]	[illegible]	3	70	97·4	4·[illegible]	[illegible]	[illegible]·7	[illegible]	[illegible]	97·7	5·2[illegible]	76	97·9	4·[illegible]
,, 10	750·9	3·[illegible]	56	54	4·4	[illegible]	7	72	97·6	4·[illegible]	[illegible]	98·0	[illegible]	[illegible]	98·4	4·5	[illegible]	[illegible]·4	4·0
,, 10	755·2	1·3	5[illegible]	52	3·[illegible]	81	6	7[illegible]	97·[illegible]	4·[illegible]	[illegible]	98·7	[illegible]	[illegible]	[illegible]·5	5·[illegible]	[illegible]	98·2	4·30
,, 11	763·2	[illegible]	53	50	3·6	80	1	7[illegible]	97·8	4·0	94	98·2	5·7	[illegible]	98·2	5·1[illegible]	83	98·1	4·3
,, 11	763·2	[illegible]	59	54	4·0	71	0	68	[illegible]·4	[illegible]	[illegible]	98·4	5·0	[illegible]	98·1	4·9	74	98·1	3·8
,, 12	751·[illegible]	1·4	56	53½	4·2	84	13	73	97·8	4·1	75	98·0	5·0	71	97·1	[illegible]	77	98·4	4·3
,, 12	751·8	[illegible]	59	[illegible]	4·[illegible]	[illegible]	2	7[illegible]	98·[illegible]	4·[illegible]	[illegible]	98·7	6·2	84	98·0	5·40	75	98·3	4·0
,, 13	751·0	0·8	5[illegible]	50	4·0	[illegible]	0	75	98·2	4·[illegible]		98·6	6·[illegible]	[illegible]	98·[illegible]	5·10	78	98·0	3·8
,, 13	751·0	[illegible]	5[illegible]	57	4·9	[illegible]	[illegible]	78	97·6	4·[illegible]	[illegible]	98·5	5·5	[illegible]	98·0	4·1	7[illegible]	98·0	[illegible]
,, 14	755·1	7·1	58	55	[illegible]·4	[illegible]	7	8[illegible]	98·2	4·1	[illegible]	98·6	5·8[illegible]	55	97·4	4·4	73	97·[illegible]	[illegible]
June 17	763·2	5·1	61	58	4·7	[illegible]	1	84		4·[illegible]	[illegible]		5·5	80		4·7	77		4·[illegible]
,, 18	764·5	1·[illegible]	[illegible]	[illegible]	5·2	[illegible]	2	7[illegible]		4·3	85		5·6	82	—	4·7	72	—	4·0
,, 20	762	[illegible]	[illegible]	[illegible]	5·[illegible]	[illegible]	[illegible]	80	—	4·0	88		5·00	94		4·8	70		4·[illegible]
,, 21	752·[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	—	4	93		5·0	97		4·8	[illegible]		3·6
,, 22	75[illegible]	[illegible]	[illegible]	62	5·0	8[illegible]	3	78		[illegible]·0	97		5·8	79		4·8	72		[illegible]
,, 23	753·[illegible]	[illegible]	65	62	5·6	83	[illegible]	80		3·9	[illegible]		5·8	[illegible]	—	4·8	66	—	3·9
,, 24	748·[illegible]	[illegible]	64½	62	[illegible]	85	2	84		[illegible]	103		5·[illegible]	83		4·8	70		3·8
,, 25	744·7	[illegible]	63½	61	5·5	8[illegible]	[illegible]	[illegible]		[illegible]	101		5·0	75		4·2	75		3·7
,, 27	749·3	4·[illegible]	61	58½	5·1	85	[illegible]	82		[illegible]·7	[illegible]		5·8	[illegible]	—	5·1	75		3·7
,, 28	744·7	[illegible]	63½	61	[illegible]	85	[illegible]				[illegible]		5·7	82		5·1	82		3·70
,, 29	74[illegible]·7	[illegible]	64	[illegible]	[illegible]·4	[illegible]	[illegible]	84		[illegible]	[illegible]		5·[illegible]	[illegible]		5·[illegible]	[illegible]		3·95
,, 30	741·2	[illegible]	[illegible]	[illegible]	[illegible]·1	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]		[illegible]	[illegible]		[illegible]	7[illegible]		3·8
July 1	7[illegible]·3	5·1	[illegible]	[illegible]	[illegible]·2	82	[illegible]	86		[illegible]	[illegible]		[illegible]	8[illegible]		5·[illegible]	82		3·7
,, 2	749·7	[illegible]	[illegible]	58	5·0	85	3	[illegible]		[illegible]	[illegible]		5·4			[illegible]	[illegible]		3·9
,, 4	750·0	1·2	[illegible]	57	4·7	[illegible]	[illegible]	8[illegible]		[illegible]·6						4·[illegible]	[illegible]		3·8
,, 5	7[illegible]	[illegible]	[illegible]	57	4·7	—		84		[illegible]·7						4·3			3·6
,, 6	[illegible]	0·4	[illegible]	56	5·[illegible]	[illegible]	[illegible]									4·4			3·7
,, 7	7[illegible]	[illegible]	[illegible]	[illegible]	[illegible]·2			7[illegible]		[illegible]·6						4·[illegible]			3·[illegible]
,, 8	7[illegible]	1·2	[illegible]	57	4·8	[illegible]	[illegible]	[illegible]		[illegible]						4·[illegible]			3·[illegible]
,, 9	755·5	[illegible]·4	[illegible]	57	4·7	82	[illegible]	84		4						4·2		—	3·75
,, 11	757·8	7	[illegible]	59	5·2	8[illegible]	6	66		3						[illegible]			3·4
,, 12	7[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	80		3·8					—	4·1			[illegible]
,, 13	7[illegible]	[illegible]·4	[illegible]	59	5·1	82	2	80		[illegible]	66		[illegible]	[illegible]		4			3·7
,, 14	760·[illegible]	1·[illegible]	62½	59	5·0	8[illegible]	2	84		[illegible]	[illegible]		[illegible]	88		4·4			3·7
,, 15	770·0	1·0	62	60	5·4	8[illegible]	[illegible]	90		[illegible]	[illegible]			[illegible]		[illegible]			3·4

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XIV. The Influence of Moisture in the Air on Metabolism in the Body.

By WILLIAM THOMSON, F.R.S.E., F.I.C., F.C.S.

(Read April 8th, 1913. Received for publication April 22nd, 1913.)

In a previous paper read by me before this Society (21st of March, 1911), I endeavoured to shew that the changes in the degree or amount of metabolism going on in the body depend largely on alterations in the state of the atmosphere, such as those of temperature, pressure, and humidity—the lowering of the temperature, or of the pressure, or of the percentage of humidity of the air producing increased metabolism, as measured by the percentage of carbon dioxide (carbonic acid gas) in the exhaled air from the lungs, whilst the raising of the pressure, or of the temperature, or of the percentage of humidity reducing the percentage of carbon dioxide (CO_2) in the exhaled air. All these atmospheric conditions are continuously and simultaneously altering, and the net results as regards metabolism may thus be deduced (within limits) from the nature and amounts of such changes. After writing this paper my attention was drawn to the very able memoir by Drs. J. S. Haldane and J. G. Priestly (Vol. XXXII., 1905, of *The Journal of Physiology*), which I read with much interest. Dr. Haldane expressed to me some doubt as to whether the degree of humidity would materially affect the percentage of CO_2 in the exhaled air, and in deference to his opinion as the highest authority in these matters I determined before submitting my previous paper for publication to further

October 31st, 1913.

investigate this subject on a larger scale than I had done previously. I therefore constructed an apparatus consisting of a lead tube, 3 inches diameter and 4½ feet long, bent at right angles at both ends, flanged at each end, and provided with lead-covered lids and rubber washers fixed down by screws when the apparatus was not in use.

This tube was filled with pumice stone and pure, strong sulphuric acid, poured in to about one-fourth its remaining capacity. An electric fan was connected with one end of this tube, through which air was drawn and delivered from the fan in a constant stream to give the supply of dried air, the air being comfortably and naturally inhaled from the stream and one exhalation, including the residual air from the mouth and throat, delivered into a small copper gas-holder, counterpoised by weights to allow the air to be put in without strain upon the lungs.

The air from the drying apparatus was unfortunately not free from taste and smell, and it was then discovered that the sulphuric acid had acted upon either the lead or on impurities in the pumice stone and had produced traces of sulphur dioxide. This was fatal to the experiment, as it had been previously shewn by me that minute traces of irritating substances, such as exist in the air of towns, had the effect of largely reducing the percentage of CO_2 in the exhaled air, as compared with the air of the country where these are absent.

I endeavoured to prevent the formation of this impurity by the addition to the sulphuric acid of a small quantity of chromic acid, but this introduced another unexpected impurity, viz., a trace of chlorine, and this appliance was finally abandoned in favour of a large bottle filled with broken glass and wetted with strong pure sulphuric acid. The warm air experiments were made by adjusting a coiled length of manganin wire inside the tube leading from the fan through which an electric current passed to provide the required temperature to the air.

The alternate breathing of dry and damp air was made by three different persons, who breathed first the damp and then the dry air about 56° Fahrenheit and afterwards damp and dry air at blood heat (98° Fah.); in each case the "damp" air was, as nearly as possible, saturated with moisture at the two different temperatures.

In these experiments the personal element enters to a large extent, and in one of the persons breathing these different atmospheres there was in some of the experiments no difference in the CO_2 present in the exhaled air between the damp and dry air (cold air). He says on these occasions he suffered from cold, and this he thinks may have accounted for it, but as a whole, even in cold air, there is with him on the average a larger percentage of CO_2 in the dry than in the damp *cold* air, and a considerable difference with him between the damp and dry *warm* air.

The total average increase for the three persons on the dry over the damp *cold* air amounted to 4 per cent., and on the dry over the damp *warm* air of 7·53 per cent.

The following gives the figures obtained for each person under the different conditions:—

CO_2 IN EXHALED AIR.

	Cold Air about 56° F.						Warm Air at 98° F.					
	Damp.			Dry			Damp.			Dry.		
	C.	W.	F.	C.	W.	F.	C.	W.	F.	C.	W.	F.
1913. March 6 ...	4·7	5·5	3·8	4·9	5·7	4·0	4·9	5·6	3·8	5·3	6·3	4·0
,, 6 ...	4·8	5·5	3·9	5·1	5·8	3·9	5·0	5·7	3·9	5·6	6·3	4·0
,, 6 ...	4·7	5·4	4·2	4·7	5·8	4·3	4·8	5·9	3·8	4·9	6·2	4·1
,, 6 ...	4·5	5·6	4·3	4·7	5·9	4·2	4·5	6·0	3·9	4·9	6·5	4·0
,, 10 ...	4·7	...	4·3	4·8	...	4·3	4·7	5·5	3·8	5·0	6·0	4·2
,, 10 ...	4·6	...	4·0	4·7	...	4·2	4·8	5·8	3·9	5·0	6·2	4·3
,, 18 ...	...	...	...	...	...	...	4·6	5·6	...	4·9	6·1	...
,, 18 ...	...	...	...	...	...	...	4·9	5·7	...	5·2	6·2	...
,, 18 ...	...	...	...	...	...	...	4·9	...	...	5·1	...	...
,, 18 ...	...	...	...	...	...	...	4·7	...	...	5·1	...	...
Average	4·66	5·50	4·08	4·81	5·80	4·15	4·78	5·72	3·85	5·10	6·22	4·10

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XV. Experiments on Abel's theory that Incombustible Dusts act catalytically in igniting weak mixtures of methane and air.

By HAROLD B. DIXON, M.A., F.R.S.,

AND

H. M. LOWE, M.Sc.

(Read January 21st, 1913. Received for publication October 25th, 1913.)

In the year 1881 Sir Frederick Abel published a series of experiments on the effect of certain dusts in the propagation of an explosion. He had examined coal-dusts from various parts of the Seaham Colliery and arranged them in the order of "sensitiveness." He showed that these coal-dusts had the property of extending an explosion in the presence of weak mixtures of fire-damp and air. Thus, although 3% methane and 97% air will not explode, this mixture exploded violently when coal-dust was suspended in it. In making these investigations Abel found that some of his most sensitive dusts, *i.e.*, those which could cause explosion of the weakest mixtures, contained less *coal*-dust than others which were not so sensitive. He was thus led to try if perfectly incombustible dusts, such as calcined magnesia, etc., could of themselves extend or aggravate the effects of a firedamp explosion. He came to the remarkable conclusion that such was the case. He says in his report,* "Mixtures of firedamp and air, in proportions bordering on those which will ignite on the approach of flame, are inflamed instantaneously if they contain in suspension only a few particles of such non-combustible dust."

* *Report of Royal Commission on Accidents in Mines, 1886.* Appendix ix.

Special experiments demonstrated that "some perfectly non-combustible powders are very little inferior to the most inflammable or sensitive of the Seaham dust-samples" in their power to bring about the ignition of an otherwise uninflammable mixture of firedamp and air.

Abel's experiments were carried out in a long wooden gallery fitted with observation windows. At one end was a Korting's blower, capable of drawing air through the gallery at any rate up to 1,000ft. per min. Near the opposite end the gas entered by a pipe fitted into the bottom of the gallery. The stream of gas and air passed a drum sieve, which on rotation allowed a supply of dust to fall into the gases below. About twelve feet further along the current passed the naked flame of a Davy lamp, or a gas flame. Air and gas in the ratio of 3% fire-damp to 97% air were passed through the gallery at a rate of 600ft. per minute. The lamp-flame produced no effect on this mixture, but when calcined magnesia was conveyed into the current, long "flares" of flame were seen on the lamp-flame, and "inflammation speedily spread through the gallery with feeble explosive effect." With 2.75% fire-damp quite similar results were produced, but the general inflammation followed less rapidly after the first flares were observed. Other incombustible dusts, such as pumice, kaolin, etc., produced similar results, but some more rapidly than others.

These results Abel explained in the following manner: "The instantaneous raising of such small dust particles to incandescence by the flame must have the effect of localising, and consequently intensifying, the heat at those points; and the ignition of the gas greatly diluted with air may thus be brought about." The action is compared to the contact, or catalytic action of platinum, which, when it "is brought into contact with a mixture of hydrogen or hydrocarbon vapour with oxygen or air, oxidation . . . . is at once established and proceeds at a rapidly accelerating rate, as the chemical activity is promoted by the accumulating heat, so that the metal is speedily raised to a temperature sufficiently high to bring the surrounding gas mixture to the exploding point."

These conclusions were apparently confirmed by an experiment of Le Chatelier. Into a long vertical glass tube (about 4ft. × 3ins.) a mixture of air and gas is introduced in such proportions that on lighting it at the top the flame hovers about in the tube or slowly descends. If magnesia dust is now dropped into the tube through a Bunsen flame burning across the top of the tube, the flame rapidly descends with the dust to the bottom.

But in the Le Chatelier experiment the dust is not suspended in the gas-and-air current, but is falling under the action of gravity through the mixture. A particle of magnesia heated by the Bunsen flame would carry a cap or small flare in its wake. If the gas mixture is non-explosive this flare does not spread, but forms a luminous tail to the falling particle: but if the mixture is just explosive, the flame spreads sideways as the particle falls, and thus forms an inverted cone of flame with the falling particle at its apex.

The importance of Abel's conclusion lies in the fact that the addition of a finely divided non-combustible dust has been adopted in some mines as a method of preventing the explosion of coal-dust, and it has been urged that if such non-combustible dusts may cause the explosion of less than 3 per cent. of firedamp, the remedy may really be introducing another danger into mines.

This aspect of Abel's work has naturally been considered by the Home Office Committee on Explosions in Mines, who have carried out experiments both in the laboratory and on the larger scale, in order to confirm or disprove the observations on which Abel founded his conclusions.

Both the experiments made by Abel and the conclusions he has drawn from them appeared open to criticism. When gas and air entering by separate pipes are drawn along a gallery a considerable time must elapse before there is complete mixture of the gas and air. Abel states that special precautions were taken to ensure mixture, and that analyses of samples taken at different places in the gallery proved that the mixture was complete. But from the description of the apparatus used it would appear that Abel depended on the length of "run"—about 16 feet—to bring about admixture. Now with a very rapid current the mixture might seem to be complete since the analysis of samples collected at the top and bottom of the gallery might be identical, but, nevertheless, the mixture might not be really homogeneous but *streaky*. In the experiments made at Eskmeals, with a gallery practically the same as Abel's, this was found to be the case. The "flares" produced in such a mixture were totally different from those formed when the gas and air had been thoroughly stirred together by being pulled through a sirocco fan. In the streaky mixture the flares were almost noiseless. They had very little charring effect and the discontinuity of the flame was evident; in the homogeneous mixture the flares became roaring flames, charring wood immediately.

With less rapid air-currents the lighter gas flowed upwards and passed along the top of the gallery. A lamp-flame below only showed a small cap, while the mixture, a few inches above the flame, was explosive. The admission of dust to such a stream of gas and air might cause ignition of the mixture above if a particle of dust heated by the flame was carried upwards a few inches. Such an ignition was seen to occur several times, and the flame then travelled along the gallery, both forwards and backwards, as described by Abel, "with feeble explosive effect." On the other hand, the admission of an incombustible dust to the current when the mixture of gas and air was complete had no effect on the cap or flare of the lamp-flame except to make it more luminous.

While these experiments were being carried out at the Home Office Experimental Station at Eskmeals, we thought it worth while to test the theoretical explanation offered by Abel of the influence of finely divided dusts on explosions.

According to Abel's explanation, the warmed dust acts catalytically like platinum to bring about the oxidation of the firedamp that surrounds each particle, and this oxidation proceeds more rapidly as the particles get heated up by the chemical action, until finally the dust particles become incandescent. So far the explanation does not appear improbable or inconsistent with known facts. But Abel supposes that the dust particles when they have reached incandescence, owing to the heat of combustion of a portion of the firedamp mixture in which they are floating, can ignite the residual firedamp, and, moreover, the flame so produced can then propagate itself into regions where this initial catalytic combustion has not taken place. This catalytic action of the dust is similar, he says, to that possessed by platinum: and just as platinum brought into contact with mixtures of oxygen with hydrogen or hydrocarbons sets up a chemical action which raises the temperature of the metal until it causes the mixture to explode, so heated magnesia acts on firedamp and air.

If will be observed that the analogy with the action of platinum on electrolytic gas is inexact. Platinum ignites electrolytic gas because the combustion of the small proportion of the gas necessary to raise the temperature of the platinum to the "ignition point" leaves a mixture which is still highly explosive in spite of its admixture with the steam formed. But in the case of the fire-damp mixture the proportion of methane is initially so low that it will not inflame in contact with a flame. The explanation therefore involves the supposition that a catalyst can cause a gas to lose part of its chemical energy as heat but at the same time makes it more chemically 'active' in propagating a flame. If magnesia dust can effect this, *a fortiori* platinum might be expected to effect it.

Two series of experiments were undertaken.

1. The direct combustion of weak mixtures of coal-gas and air with and without non-combustible dust, on a small scale, in order to see whether any difference in the propagation of the flame could be noticed.

2. The heating of a length of platinum wire in weak mixtures by an electric current to show that by this means a non-explosive mixture could not be made to explode, but rather that an explosive mixture thus treated would become non-explosive owing to the combustion on the platinum wire.

1. *Direct experiments with calcined magnesia dust.*

The apparatus consisted of a thick vertical glass tube, 37cm. long by 4cm. wide, closed at the bottom end, and fitted at the top with a cork. The cork carried a leading tube through which a previously made mixture of air and gas could be introduced from a gas holder. The leading tube went to the bottom of the wide tube, and was drawn out to a narrow jet. Dust could be placed in the bottom of the tube, and on introducing the gas it was blown about in the form of a light cloud. Through the cork two brass wires were inserted, by means of which an electric discharge could be passed at any point in the tube. A hole was also bored in the cork as an exit for the gas, and was left open during all the experiments.

A mixture of 15% of Manchester coal-gas with 85% of air was made and stored in a gasholder over water. A portion was passed into the apparatus and an electric discharge passed between the brass wires. The gas exploded violently and the cork was blown out. In the next experiment magnesia dust was introduced, the gas blown in and exploded as before. In this case a yellow flame was produced instead of a blue one, but no other difference was observed.

Nine per cent. coal-gas sparked near the bottom of the tube is immediately burnt up, but when sparked near the top of the tube the flame slowly travels downwards, with a peculiar "jelly-fish" flame at a rate of several cm. per second. The introduction of dust only made the flame more luminous, but did not otherwise affect it.

Eight per cent. of gas sparked at the bottom of the tube was burned completely, but the flame would not travel downwards when sparked at the top. The same facts were observed when dust was present.

Seven per cent. of gas behaved similarly to 8% in all particulars.

Six per cent. appeared on the lower limit of explosiveness, often not exploding with the first spark, but only after two or three discharges. Dust did not make it explode any more easily or more frequently.

These experiments show conclusively that on the small scale at least the presence of calcined magnesia does not have any such effect as that described by Abel. That it does not alter the lower limit of explosiveness is shown by the 6% mixture; and that the character and rate of propagation of the flame is not altered is shown by the experiments with the 8% and 9% mixtures.

2. *Effect of Electrically Heated Platinum Wire on Weak Mixtures.*

The apparatus used consists of a glass tube 3″ diameter 10″ long, fitted with a rubber stopper bored to fit two glass tubes and four brass wires. The glass tubes are fitted with taps for leading in the gas. The brass wires are connected to the secondary circuit of an induction coil, and a spark can be made to pass between the ends. The wires are connected through a variable number of lamps to the 200 volt lighting circuit, thus heating a platinum wire.

A short platinum wire (4·4 cm.) heated by a current which in air brings it to a full red heat, when immersed in a mixture of coal-gas and air containing 8% of coal-gas, glows very brightly, and may even get so hot as to fuse. As soon as the wire reaches a certain temperature (the ignition point of the gas), the gas explodes.

When the same wire is only gradually heated, by a regulated current, not allowing it to get above a dull redness for about a minute, it may then be heated to whiteness, or an electric spark may be passed without exploding the gas. The reason for this is that so much oxidation of the hydrogen present has taken place that the composition of the mixture has been reduced below the explosive limit (about 6% coal-gas).

A platinum wire 1 metre long, heated by a current, which in air brings it to dull redness, when heated in 8% mixture glows brightly but does not explode the gas.

So long a wire has, of course, a much greater surface than the shorter wire, and consequently a much larger proportion of the gas will burn on the wire before it reaches the ignition point of the mixture. So rapidly is the composition of the mixture reduced that a spark passed several seconds later would not explode the gas.

Experiments were tried to find exactly how long was necessary for the gas to be treated in order to just bring it to a non-explosive point. The platinum wire was heated by a current sufficient to bring it to very dull redness in air. When heated in an 8% mixture it glowed brightly, and when a spark was passed eight seconds after the heating current had been turned on, it was found that the gas would not explode. When the spark was passed seven seconds after the heating current had been turned on, a feeble explosion resulted.

A sample of 8% coal gas taken at a different time was reduced to a non-explosive point in seven seconds but not in six seconds.

On the other hand, a sample of 6% coal-gas which was not capable of being exploded by a spark was tried in order to see whether by any heating of the platinum wire it could be made to explode.

It was found that no explosion was produced either by gradually heating the wire as in previous experiments or by suddenly heating it, even to a white heat. The gas could not be exploded by a spark passed immediately after the current had been switched on.

In this experiment the ideal conditions demanded by Abel have been reproduced. The platinum surface, to represent the dust has been heated, combustion takes place on its surface, thereby intensifying the heat, but it does not bring about (as Abel said) the general ignition of the gas greatly diluted with air.

From the above experiments it may, therefore, be concluded :—

1. That the presence of a non-combustible substance does not cause the ignition of a weak gas mixture which is not ignited by the flame or a spark.
2. That if any local combustion takes place on the dust, causing self-heating, such an action causes the remainder of the gas to be less explosive, and not more explosive.

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XVI. The Root-apex and Young Root of *Lyginodendron*.

By F. E. WEISS, D.Sc., F.L.S.

(*President of the Society.*)

(*Read November 26th, 1913. Received for publication September 9th, 1913.*)

The great interest which has centred round *Lyginodendron*, the fronds of which are so conspicuous an element of the plant remains of our Coal Measures, is due to the gradual recognition that, far from being an ordinary Fern, it and its allies occupied in many respects an intermediate position between the Ferns and the Flowering plants. The first points of resemblance to the lowest groups of the latter, the Cycads, were discovered by Williamson and Scott, who investigated their vascular anatomy. They were considered to be of sufficient importance to warrant the separation of *Lyginodendron* from the Filices and the placing of this plant and the nearly allied *Heterangium* in a new group, the name of which, Cycadofilices, indicated the intermediate position of the plants as a link between Cycads and Filices. Since the discovery by Oliver and Scott that *Lyginodendron* bore like the Cycads true seeds, the term Pteridospermae has appropriately superseded that of Cycadofilices as the name of the group which is now known to have included a much larger number of fern-like plants of our Coal Measures than was at first thought to be the case. For this reason the term Pteridospermae is, on the whole, preferable to that of Cycadofilices, for it may well turn out that some of the seed-bearing ferns had little direct affinity with the Cycads. As I have

November 29th, 1913.

pointed out elsewhere, Professor Chodat,[1] of Geneva, throws some doubt on the Cycadian affinities even of *Lyginodendron* and *Heterangium*, and it becomes of interest, therefore, to investigate detailed points of anatomy in further elucidation of the structure of any of the members of the Pteridospermae.

Among some of the most beautifully preserved plant remains from the calcareous nodules found in and immediately above some of our Lancashire coal-seams are the young root-tips of *Lyginodendron*. One of these was figured by Stopes and Watson in their memoir " On the Present Distribution and Origin of the Calcareous Concretions in Coal Seams," (See *Pl., Fig.* 1),[2] as indicating the very perfect preservation of delicate growing tissues of some of the Coal Measure plants. I have for some time been collecting preparations of these root tips with a view of their further study. In most of our recent ferns belonging to the Leptosporangiatae, the root-apex is occupied by a single cone-shaped apical cell, while in the Marattiaceae, which are now generally regarded as some of the more ancient of living ferns, a group of initial cells is responsible for the further growth of the root. The Osmundaceae occupy in this respect an intermediate position between the bulk of the Leptosporangiate Ferns and the Marattiaceae. For as Bower has shown,[3] in the roots of Osmunda and Todea various irregular and intermediate conditions have been found between the Marattiaceous type, with four prismatic initials, and that of the Leptosporangiate Ferns, where only one occurs.

[1] Chodat R. Les Pteropsides des temps paléozoïques. *Archives des Sciences physiques et naturelles, Genève.* Tome XXVI., 1908.

[2] *Phil. Trans.*, vol. 200, 1908.

[3] Bower, F. O. *Annals of Botany*, vol. V., 1889, p. 109, and also "The Origin of a Land Flora," 1908, p. 650.

From the earlier specimens of young *Lyginodendron* roots which I examined, I concluded that it was probable that they agreed with the Marattiaceous type, but the further specimens I have now had an opportunity of examining, have led me to somewhat modify my view. Even in the case of existing plants, it is not always easy to be certain whether a growing point possesses a single apical cell, and, in the case of fossil root tips, of which only one longitudinal section is usually available, it is still more difficult, because it is often impossible to tell whether the section under examination is strictly median or not. In the case of a transverse section, too, if a group of initials appear, it is quite possible that the section has been taken below the actual apical cell.

Fig. 1 is an accurate camera lucida drawing of one of the best longitudinal section through the root-apex of *Lyginodendron*, and is the same as the one referred to above as figured by Stopes and Watson. (*Phil. Trans.*, vol. 200.) It is on slide R. 646 in the collection of the Manchester Museum, formerly in the possession of the late Mr. Thomas Hick. The slide is prepared from the well-known Dulesgate material, and contains numerous fragments of stem, roots, and petioles of *Lyginodendron*. The total length of the portion of the root-tip, including the various layers of the root cap is only ·65 mm., the widest portion of the root being ·21 mm., while near the apex the root narrows down to ·07 m.m.

To enable an estimate to be made of the extreme delicacy of these lateral roots, it may be stated that an average-sized root-tip of *Aspidium* is from two to three times the width of this particular root of *Lyginodendron*. Nor is this one the most delicate, for other longitudinal sections measured about the same distance behind the apex varied from ·16 to ·24 mm. in diameter.

Some justification is perhaps needed for assigning this and similar root-tips to *Lyginodendron*. Their fairly constant association with undoubted older roots of *Lyginodendron* would in itself be insufficient evidence, were it not strengthened by a very large series of rootlets showing every stage of transition, from root-tips in which no vascular tissue is as yet to be seen, to fully mature rootlets showing the typical structure of *Lyginodendron*. In addition, we find in all the rootlets evidence of the differentiation of the more external into an outer large-celled tissue and an inner small-celled coitex, the latter possessing numerous secretory sacs with dark contents. The presence and position of these latter cells renders the identification of the rootlets a fairly easy matter.

The first examination of the root-apex shown in *Fig.* 1, particularly as on focussing somewhat deeply a line marking the limit of the plerom cylinder becomes visible, led me to infer that the roots did not possess a single apical cell; but a closer examination has caused me to believe that the section is not sufficiently median to enable this conclusion to be drawn from it. In the upper part of the rootlet, which, as can be seen from the smaller diameter, is cut tangentially, a secretory sac is shown in the middle portion of the section, which indicates that the section here shows only cortical tissues and not those of the central cylinder. Nearer the apex, though the limits of the central cylinder can be made out by focussing more deeply, the same still holds good, and the small dark cell near the centre of the section is still one of the secretory cells of the cortex and not part of the central cylinder. It follows, therefore, that the evidence of this somewhat tangetical section will not warrant the conclusion that the root did not possess a single apical cell.

On the other hand, the series of layers of the root-cap is rather suggestive of these having been cut off seriatim from a single apical cell. It will be seen that the root-cap is very massive, and extends a considerable way back from the apex. In the section under consideration the cells of the root cap have become much compressed and blackened, probably due to their disorganisation, except near the root-tip, where they are distinct and can be seen to be of large size. On another root tip (*Fig.* 2), where they are better preserved, the cells can be seen to be two to three times the diameter of the meristematic cells and many times the length.

Among the more numerous transverse sections through young root, the one represented in *Fig.* 3 is so close to the apex that it includes the remains of the disorganised root-cap to the outside. Only ·15 mm. in diameter, it occurs on a slide (R. 617), crowded with remains of *Lyginodendron* roots of all ages. The transverse section of this root apex shows four very distinct external layers of cells, which can readily be identified with the corresponding tissues of the older roots. The outermost two layers, consisting of large cells closely set together, are the two characteristic layers of the *Lyginodendron* roots, called exodermis by Scott. The outermost, as will be seen from an examination of the longitudinal sections, might be considered to be of epidermal nature. The third layer, from the brown contents of some of its cells, can be easily identified with the inner cortex. On the inside of this tissue small intercellular spaces can be observed in some parts of the section. Next comes the endodermis, composed of six very clearly defined cells. The number and very regular arrangement of these cells reminds one of the condition of young fern roots, and is very suggestive of the origin of the tissues from a tetrahedral apical cell. In older parts of the root, of course, the number of endodermal cells becomes materially increased, but in young roots, where the number six is exceeded, nine and fifteen are not uncommon though they do not always run in multiples of three. But this number six, a little way behind the apex, is what one would expect in a root with a three-sided apical cell, though it might also occur in a root with a group of three cells, which is, of course, also a possibility. The cells of the central cylinder are very small compared with those of the outer layer. In the transverse section under consideration they are somewhat defective on the upper side of the section, where only the most centrally lying cell is clearly visible, but on turning the slide over the other walls can be more or less clearly seen. It will be seen from the drawing of them made with the camera, that they are somewhat irregularly arranged. They are only small in number, and their general arrangement is more reminiscent in this early stage of such a fern root as Azolla rather than of Aspidium.

In the young stage represented in *Fig.* 3 no vascular tissue is formed, but it becomes differentiated very early, one protoxylem group making its appearance first, to be followed shortly by a second one (*Fig.* 4), the two groups becoming subsequently connected by metaxylem elements. Both these stages are frequently met with among the sections of delicate rootlets. Triarch arrangement is only found in rootlets of greater diameter. Whether triarch roots arise from the delicate diarch ones it would be difficult to say; but in one of the smaller triarch roots there were indications of one of the protoxylem groups having arisen later than the two others. In very young roots the tissues of the central cylinder are often defective, and frequently only a single or a couple of tracheids are found adhering to the inner wall of the endodermis, the other cells having perished (*Fig.* 4).

One other point seems worth mentioning. I have endeavoured to trace the origin of some of these delicate lateral rootlets from the roots from which they sprang, and in a number of instances have found such connections. My object was to find out, if possible, the orientation of the diarch plate of the rootlet to the axis of the parent root. In the few cases in which it was possible to observe this connection the diarch plate was found to be vertically placed. Owing, however, to the defective preservation of the tissues of the central cylinder referred to above as well as to the frequent obiquity of the section, it was not easy to select a very convincing section for reproduction. I give, however, a camera lucida drawing of one (*Fig.* 5), in which the vertical plate of tracheids of the emerging rootlet is fairly clearly seen. The interest of this particular point in the anatomy of the rootlet lies in the fact that Van Tieghem[4] has pointed out that one of the differences between the vascular Crygotagams and the Phanerogams is to be found in the orientation of the plate of tracheids of lateral rootlets. In the former the plates stand horizontal, in the latter vertical. *Lyginodendron* would, therefore, in this particular agree with the Flowering Plants rather than with the Ferns.

I am aware that the facts concerning the root-apex of *Lyginodendron* given in this communication may appear to be somewhat meagre, but it seemed well to put them definitely on record, as they are the result of an examination of a very large number of rootlets, and so far no detailed account of them has been given. Scott[5] refers to the apparent absence of a definite apical cell which I had considered probable on first examination of the only section then known to me which passed thrcugh

[4] Van Tieghem. Traité de Botanique. 2e Edition, 1891, p. 705.

[5] Scott, D. H. Studies in Fossil Plants, 1909, vol. ii., p. 387.

the root-tip. But, as I have endeavoured to show in this paper, this and other longitudinal sections through root-apices which I have since examined seem to me not to be quite median, and as the meristematic cells are small, it is more probable that the absence of a definite apical cell in the section is to be attributed to the somewhat tangential nature of the section than to the absence of a single apical cell. This conclusion is based upon the examination of transverse sections which pass close to the apex, and in which the regularity in the arrangement of the tissues suggests a structure of the apex resembling that of recent Leptosporangiate Ferns. Of course it is possible that the more delicate lateral rootlets like those described above possessed a single apical cell, while the more massive adventitious roots emerging from the stem were provided with a group of initials. Our knowledge of root-tips of the recent Osmundaceae makes such a suggestion possible. As attention has now been drawn to the occurrence of these young root tips, it is probable that other observers may be able to supplement the information given above, and truly median longitudinal sections may be found from which we can conclusively prove the existence or non-existence of a single apical cell.

EXPLANATION OF PLATE.

Fig. 1.—A longitudinal section of root-apex of *Lyginodendron* from preparation in the Manchester Museum. Slide R. 646. × 125. The section is slightly tangential. Indications of the central cylinder are visible, but the actual initial or initials of the apex are not seen. *s.c.* = secretory cells of inner cortex ; three of these are nearer the root-apex. *r.c.* = root cap. This massive root cap seems to be made up of a series of layers.

Fig. 2.—Longitudinal section root-apex of young root-apex of *Lyginodendron* from Slide R. 614. × 150. This section shows the large size of the cells of the root cap. The section is tangential, so that none of the secretory cells of the niner cortex are seen.

Fig. 3.—A tranverse section of rootlet of *Lyginodendron* taken very near the apex, as can be seen from the remains of the root cap (*r.c.*) surrounding it. Slide R. 617 in the Manchester Museum. × 244. The outermost two layers are the characteristic exodermis. *s.c.* = secretory cells of inner cortex. Note intercellular spaces *en.* = endodermis consisting of six cells.

Fig. 4—Section across a young rootlet, showing distinctly the exodermis, the thin walled inner cortex, with secretory cells, the entodermis and the two groups of protoxylem of the diarch root.

Fig. 5.—Longitudinal section through a rootlet of *Lyginodendron* at a point where a lateral rootlet is cut across. A vertical row of tracheids of this diarch rootlet is seen near the centre of the section Slide R. 1025-6 in the Manchester Museum. × 125.

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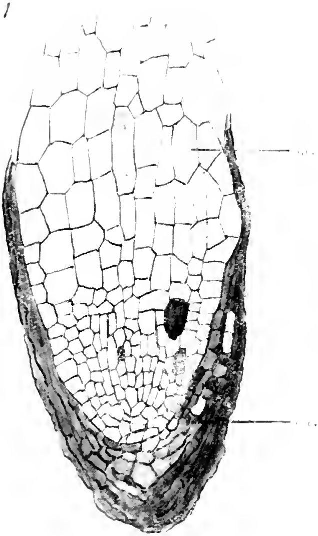

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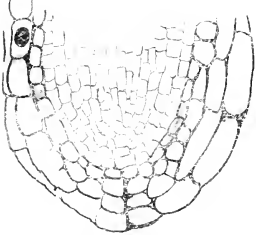

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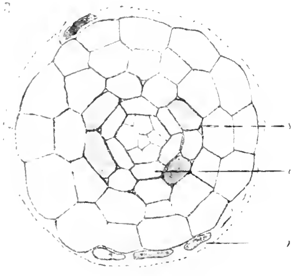

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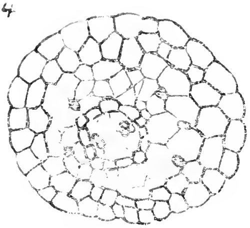
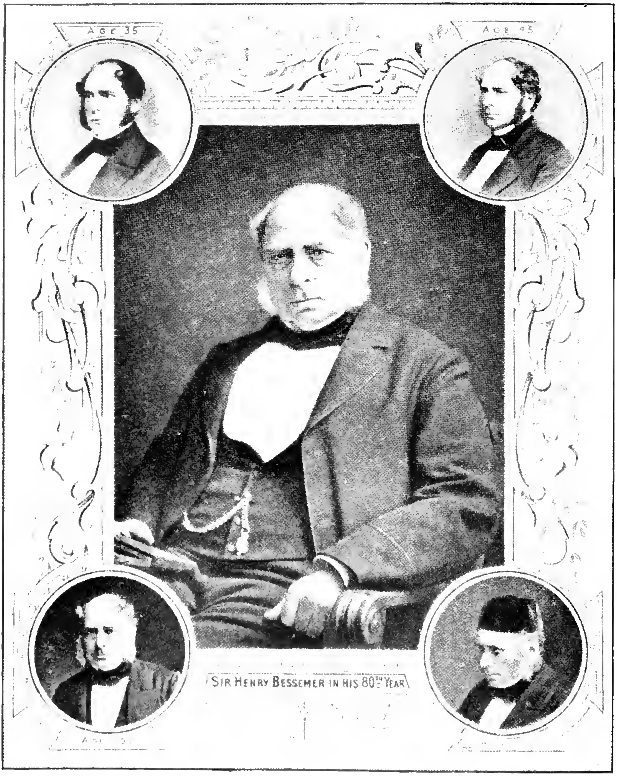




SIR HENRY BESSEMER, F.R.S.
1813-1898.

XVII. Bessemer, Göransson and Mushet: A Contribution to Technical History.

By Ernest F. Lange,
M.I.Mech.E., Assoc.M.Inst.C.E., M.I. & S.Inst., F.C.S.

(Read May 6th, 1913. Received for Publication, August 29th, 1913.)

No one can study the history of the Bessemer process of steel-making without feeling that no adequate attempt has ever been made by any writer on the subject, including Bessemer himself, to place, in their proper light and exact significance, the parts played by Göransson and Mushet in the successful development of this epoch-making process.

Shortly after the death of Sir Henry Bessemer in 1898, I discussed this matter with my father-in-law, the late R. M. Daelen, of Düsseldorf, the well-known authority on iron and steel manufacture, who was acquainted with all the three men above mentioned—in fact, intimately so with Bessemer and Göransson—and he agreed that a useful contribution to the history of metallurgy could be made by preparing an exact analysis of the facts surrounding the transition stage of the Bessemer process from its early failure into a success, so important in its results as to rank next to the discovery of the steam-engine in its effects upon the civilization of the world.

I had already collected matter bearing upon this subject in connection with a lecture upon the Bessemer process in Crewe, which I delivered in the December of

1894, and this was then added to by my father-in-law who, by reason of his honourable position, was in close touch with the affairs of the iron and steel world, but up to the time of his death in 1905, we had neither of us worked up the material thus available into the form of a complete argument.

Since that year, I have frequently considered the feasibility of presenting this in the form of a Paper before one of the learned Societies, but the small amount of leisure which I have, afforded little opportunity for the necessary preparation. However, in response to pressure on the part of some of my friends, and in remembrance of the fact that this year is the centenary of Henry Bessemer's birth, I have more recently endeavoured, in what spare time I could find, to fashion my notes into a condensed but readable sequence, and in asking this Society to do me the honour of allowing me to present them within the walls sacred to the memory of Dalton and Joule, I feel that the subject matter of my discourse which is intimately, indeed principally, concerned with the application of the exact sciences to the service of industry, will be appropriate to the surroundings.

Henry Bessemer was born on January 19th, 1813, at Charlton, near Hitchin, in Hertfordshire. From his father, Anthony Bessemer, he seems to have inherited his love of science and mechanical skill. Anthony Bessemer was undoubtedly a man of genius. He was born in London, and, at an early age, emigrated with his parents to Holland. Here he was brought up as a mechanical engineer, and, at the age of twenty-one, he went to seek his fortune in Paris. We find that five years later he was elected a member of the French Academy of Sciences, for improvements in the microscope. Later he was employed at the Paris Mint, where he made and worked the first copying-machine for reproducing engraved work. At the time of the French Revolution, he fled to England, narrowly escaping with his life. Here he married, and settled in the little village of Charlton. Here, also, he started the trade of type-founding, and, together with his partner, Caslon by name, built up a flourishing business. Here in this village, Henry Bessemer was born, and from his earliest youth was brought into contact with the skilled mechanical work of the type-foundry. When he was seventeen years of age, the family removed to Camden Town, in the north of London. Here he made the acquaintance of the Allens and Longsdons, an event of great import for his future.

Bessemer made his first step towards independence by turning to commercial account his hobby of the making of art-castings. When twenty years old, he was already an exhibitor at the Royal Academy.[1] From this he passed on to the production of decorative stamping-work, in which his father's experience at the Paris Mint proved useful. We find him also at this date considering a method of preventing the transfer of Government stamps from old to new deeds, a practice which, he was informed, was causing the Stamp Office a loss of revenue of about £100,000 a year. This he accomplished by his invention of the method of dating stamps by perforation. Not having patented this invention, he received no reward for the same, in spite of official promises. Forty-five years later, when wealthy and celebrated by reason of other achievements, he was knighted for the invention thus so unjustifiably appropriated. His unfortunate experience with the Stamp Office turned young Bessemer from an amateur into a man of business, and we find him making money by many ingenious mechanical processes. At the

[1] See Ure's "Dictionary of Arts, Manufactures and Mines."

age of twenty-one, he married the daughter of his friend Richard Allen—a happy partnership that lasted until her death sixty-three years later, in 1897, within a year of his own death.

Following upon his marriage, Bessemer invented his beautiful and profitable process for the production of imitation Utrecht velvet,[2] and in 1843 he started his secret process of manufacturing bronze powder, which, with the help of his brother-in-law, Richard Allen, junior, was to run for nearly forty years, and which provided the money for his early steel-making experiments.

We now find Bessemer engaged in improvements in sugar-making machinery and glass-making machinery, which were exhibited in the International Exhibition of 1851.[3] At this time, he had as confidential partner in his drawing office, Robert Longsdon, who afterwards became his brother-in-law.[4]

THE BESSEMER STEEL PROCESS.

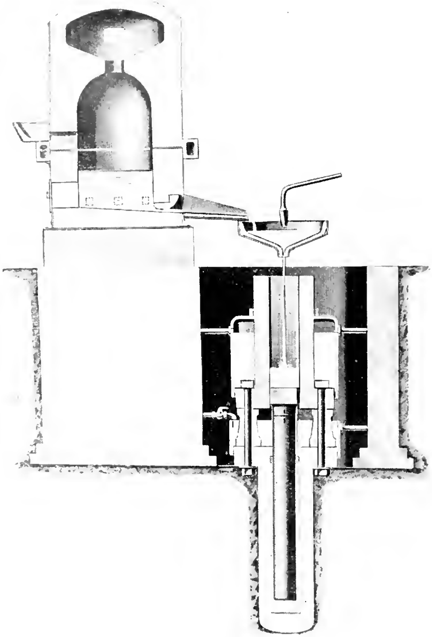
The history of the Bessemer process begins with the invention, in 1854, of a rotating projectile for the, then, smooth-bore guns, and which the War Office, with its usual conservatism, ignored. Bessemer, however, succeeded in interesting Louis Napoleon, later Napoleon III, in his invention, and money for experiments was advanced by Napoleon, he himself being the author of an important work on artillery.[5]

[2] Samples of some of the earliest pieces of this imitation embossed Utrecht velvet came into the possession of a friend of the family—Sir William Bailey, of Salford—and some correspondence relating to the same will be found in the Appendix, Note 1.

[3] See Appendix, Note 2.

[4] Another Longsdon (Alfred) ultimately became the representative of Krupp, of Essen, in England. On the death of Alfred Longsdon—then a director of Krupp—August Reichwald became the sole representative in the United Kingdom for the Krupp Company.

[5] Napoléon, Études sur le passé et l'avenir de l'artillerie. Paris, 1846.



Section of early Bessemer Converter as worked at Baxter House, St. Pancras, 1856, with bottom-stoppered ladle and hydraulic ingot mould.

Reproduced by kind permission of "Engineering."

A chance remark by Commandant Minié—the inventor of the rifle of that name—who attended one of the experiments—that the new projectile would require a better gun than one of cast-iron, led Bessemer on to the path of experiment, which culminated in his process of forcing air through molten pig-iron, and, by thus removing the silicon, manganese, and carbon, to convert the same into steel.

Bessemer first began by fusing pig-iron in a reverbatory furnace and then adding broken bars of blister steel.[6] This furnace was specially constructed to give a high temperature, and, in order to obtain a better combustion of the gases, a hollow bridge, with numerous perforations, was employed to admit of hot air being introduced. This hot air was found to produce decarburization of the metal, and an accidental occurrence of almost complete decarburization of the exterior of some pieces of pig-iron in this way, led Bessemer to the idea of trying to decarburize molten iron by a stream of air[7]. To try this, he brought a perforated pipe into a crucible containing molten iron, but without removing the crucible from the furnace. In half an hour the blowing was found to have converted the grey pig-iron into soft malleable iron. Following upon this, Bessemer conceived the idea of trying the effect of blowing air through liquid cast iron without external heat, and, to do this, he fitted up a simple form of cylindrical converter about 4ft. in height and fitted with six horizontal tuyeres round the bottom of same. This converter, in a somewhat later and more elaborate form, is shown in *Pl. I.* A blast of

[6] It is probable that Bessemer knew of Fairbairn's experiments of melting scrap-iron with pig-iron. Fairbairn, however, did this in a cupola, and the resultant product therefore contained too much sulphur.

[7] Bessemer was aware of Nasmyth's patent, May 6th, 1854, for blowing steam through iron in a puddling furnace.

10 to 15lbs. pressure was now turned on and about 7 cwt. of molten pig-iron introduced. A violent reaction took place, and, at the end of about 20 minutes, the converter was tapped out into a mould and the metal was found to be an ingot of malleable iron. An hydraulic arrangement for forcing this ingot up out of the mould was connected to this converter. This is clearly shown in the illustration. Bessemer had now proof of the fact that molten pig-iron could, without the employment of any further fuel or skilled manipulation, be raised within half an hour to a temperature hitherto unknown in metallurgy, with removal of the carbon, silicon and manganese, and that, therewith, an industrial revolution had been born. A similar experiment was shown by Bessemer to his friend George Rennie in the August of 1856, who was so astonished at the result as compared with the long and expensive processes of steel-making as then practised, that he begged Bessemer to at once publish his discovery, by writing a paper upon the same for the forthcoming meeting of the British Association at Cheltenham. This Bessemer did, entitling the same "The Manufacture of Malleable Iron and Steel without Fuel." How fantastic this title must have appeared to the iron and steel trades is shown by a story told by Bessemer of an experience on the morning of the day of the meeting. Bessemer says in his autobiography[8] :—

"On the following morning (August 13th), while finishing "my breakfast at the hotel, I was sitting next to Mr. Clay, the "manager of the Mersey Forge, to whom I was well known, "when a gentleman who turned out to be Mr. Budd, a well-"known Welsh ironmaker, came up to the breakfast table "and, seating himself opposite to my friend, said to him, "Clay, "I want you to come with me into one of the Sections this "morning, for we shall have some good fun. There is actually a

[8] Sir Henry Bessemer, F.R.S., "An Autobiography," published by *Engineering*, 35 and 36, Bedford Street, Strand, W.C., 1905.

"fellow come down from London to read a paper on the "manufacture of malleable iron without fuel."

"Oh," said Mr. Clay, "that's just where this gentleman and I are going."

The paper created the sensation that might have been expected. The first speaker in the discussion was James Nasmyth, who referred in glowing terms to the novelty of the process, whilst he was followed by Mr. Budd, above referred to, who made the *amende honorable* by generously offering Bessemer an opportunity of testing the process commercially at his ironworks free of cost.

On August 27th, fourteen days after the publication of the paper in *The Times* newspaper, the proprietors of the Dowlais Iron Works, on the recommendation of their chemist, Edward Riley, took out the first licence for the process, and, in less than one month, sales of royalties to the amount of £27,000 had been made. Surely no other invention ever so quickly revealed such visions of riches, and yet, in the very hour of triumph, the elements of disaster were gathering. This was due to the fact that the material used by Bessemer for his first experiments, and ordered by him merely as pig-iron, was supplied by some London founders from a stock of good-quality grey Blaenavon iron, and, all unknowingly, suitable for the requirements of the Bessemer process, as regards the generation of heat in the blowing operation. It was, therefore, imagined by Bessemer and his first licensees that any kind of pig-iron was suitable for the pneumatic process, and the ironmakers took their own brands to experiment with, not knowing that what was right for the refinery or puddling furnace was wrong for converters lined with silicious materials. The result was that the first licensees found that all they obtained was a decarburized, rotten, red- and cold-short, and, therefore, worthless material. In this connection I cannot do better than quote here a letter by Mr. Edward Riley, a copy of which Mr. Harold Jeans of the *Iron and Coal Trades Review* has kindly sent me :—

To the Editor of the *Iron and Coal Trades Review.*

Sir,—In *The Times* of September 25th, 1856, Mr. Henry Bessemer's (late Sir Henry Bessemer's) paper on the manufacture of wrought iron without fuel, read on the previous day at the meeting of the British Association at Cheltenham, was published.

Mr. Menelaus, of Dowlais, on the afternoon of the same day, drew my attention to the paper, and asked my opinion about it. I thought it feasible, and, the same evening, I made an experiment in a small Sefstrom furnace, the interior being about the size of an ordinary tall hat, lined with fire-clay, and having eight small holes about an inch from the bottom. To these holes were adapted, from the blast-furnace pipes, the ordinary blast, from 2½ to 3lb. pressure. About 7 to 8lb. of molten foundry iron was run into the furnace, and the blast was turned on. Distinct evidence of chemical action took place, although, owing to the small mass of metal, the iron soon became solid.

This small experiment was considered so far satisfactory that Mr. Menelaus requested me to erect a small furnace on a larger scale, so as to operate on a few hundredweights of metal.

I then built a small furnace 1 foot 9 inches square in 9-inch brickwork, with a tap-hole, and about 6 feet high, close to No. 18 furnace at the ironworks, so as to ensure a supply of all-mine foundry iron. About two inches from the bottom, on each side of the furnace, I inserted two tuyeres, made of ¾-inch wrought-iron gas-tubing, the nozzle being reduced to a bare ½ inch. After drying the furnace, about 2 or 3 cwt. of molten foundry iron was run in. The ordinary blast was then turned on, and, to the surprise of numerous spectators—I cannot remember if Mr. Menelaus was present, Mr. Edward Williams certainly was—a violent chemical action took place. The temperature was intense, and it was evident that all the reactions took place that are now so well known in the ordinary Bessemer process.

I stood on the top of a hot-blast stove and watched the process for, as far as I remember, fully half an hour. The furnace was then tapped and the contents were run out. They appeared to be nothing but cinder. Underneath the cinder was a small mass of metal--I should say not more than from 14 to 21lb. This was taken to the mill and rolled into a bar 1½ inch by ⅝ (five-eighths) inch. A piece of the same bar I have still in my possession. The bar could be bent to a fair extent, but broke with a crystalline fracture. Analysis gave :—

	Per Cent.
Carbon	Trace.
Silicon...	0·099
Sulphur	0·068
Phosphorus	0·753
Arsenic	nil.
Manganese	nil.
Copper	nil.
Iron	99·080
	100·000

The bricks by the tuyeres were completely eaten away, and it was evident that if the operation had been carried on much further the metal would have penetrated the brickwork, so energetic was the chemical action that took place.

This experiment was considered so promising, that Mr. Menelaus instructed me to erect a small furnace—the plans being supplied by Mr. Henry Bessemer—similar in every respect to the experimental furnace that was in use at Baxter House, St. Pancras.

In the meantime I had personally seen the experiments made at Baxter House, and had had opportunities of testing and examining the metal made at Dowlais, as well as that made at the London St. Pancras Works.

It was at once apparent that no phosphorus was separated from the pig. The pig used by Mr. Bessemer was the best Blaenavon foundry-iron. The iron made from this worked fairly well for a time, and I almost succeeded in making a half-inch nut. Unfortunately, just as it was finished, it broke in two, proving that the metal was not workable.

Mr. Bessemer's opinion was that the most common forge-iron could be used. The furnace referred to above was erected between Nos. 16 and 17 blast-furnaces, both working on ordinary forge-iron, chiefly used to produce common puddle-bar for the manufacture of rails.

A small blowing cylinder was erected to supply a blast of from 7 to 10lb. pressure to the furnace. After warming up the furnace, from 7 to 8 cwt. of molten forge-iron was run in, and the metal was blown from 20 to 25 minutes. The usual reactions took place, the cinder was run off, and a small ingot, about 6 inches square, was cast. This was at once taken to the forge and put under a crocodile-squeezer.

The result was that the ingot all went to powder, amidst the hearty cheers and shouts of the puddlers. I succeeded, however, in securing a corner of the ingot, and, with very careful manipulation under the squeezer, made a small bloom. This was rolled in the puddle-bar rolls into a bar 3 to 4ft. long. On testing it, I found it broke lengthways and crossways, and was as brittle as glass.

The experiment was considered so unsatisfactory, that no further experiments were carried out, although I had a very strong opinion at the time that ultimately the process would be successful, as, from experiments I had made, I found that, on melting the best cable bolt-iron in pots, the iron after fusion became very red-short and could not be worked unless some metallic manganese was added to it.

The popular opinion at this time, after the experiments made, was very unfavourable to the Bessemer process.

A rail was rolled at Dowlais from an ingot made by Mr. Henry Bessemer at Baxter House. It rolled perfectly, but broke while hot.

I have a piece from the broken end, and enclose an analysis of the rail, and also one of Blaenavon foundry-pig, which was the metal used for the blow.

FIRST BESSEMER RAIL ROLLED.

	Per cent.
Carbon ...	Trace.
Silicon ...	Trace.
Sulphur ...	0·235
Phosphorus ...	0·516
Arsenic ...	nil.
Manganese ...	nil.
Copper ...	nil.
Iron ...	99·249
	100·000

PIG USED FOR ABOVE BESSEMER RAIL.[9]

Carbon ...	3·40
Silicon ...	1·36
Sulphur ...	0·07
Phosphorus ...	0·29
Manganese ...	0·28

Yours, etc.,

EDWARD RILEY.

2, City Road,
London, E.C.,
March 21, 1898.

[9] The analysis of pig-iron given above is not from the actual pig-iron used: it is an analysis of the best Blaenavon foundry metal, such as was used by Mr. Bessemer in his experiments at Baxter House.

In his autobiography Bessemer gives a somewhat naïve account of the failures that followed his Cheltenham paper.

The first converters were erected at Messrs. Galloways' at Manchester, at Dowlais in Wales, and at the Govan Ironworks at Glasgow, and, in each case, the results of the trials were most disastrous. Bessemer says:—

> "The ordinary pig-iron used for bar-iron making was "found to contain so much phosphorus as to render it "wholly unfit for making iron by my process. This startling "fact came on me suddenly, like a bolt from the blue; its "effect was absolutely overwhelming. The transition from "what appeared to be a crowning success to one of utter "failure well nigh paralysed all my energies. Day by day "fresh reports of failure arrived; the cry was taken up in "the press; every paper had its letters from correspondents, "and leaders, denouncing the whole scheme as the dream "of a wild enthusiast, such as no sensible man could for a "moment have entertained. I well remember that one "paper, after rating me in pretty strong terms, spoke of my "invention as 'a brilliant meteor that had flitted across the "metallurgical horizon for a short space, only to die out in "a train of sparks, and then to vanish into total darkness.'[10]
>
> "I was present at some of these trials, and saw the "utter failure that resulted with the quality of metal operated "upon. It is a curious and scarcely credible fact that not "one of the ironmasters who had previously felt such "abundant confidence in the success of the process, as to "back their opinions with large sums of money, took any "trouble whatever or offered any practical or scientific "help towards getting over this unlooked-for difficulty. They "all stood by, mere passive and inert observers of the fact, "not one of them lifting up a finger, or stretching out a hand "to save the wreck."

At this juncture, Bessemer saw that it was necessary for him to know what the constituents of commercial pig-iron were, and he, accordingly, called in the aid of Dr.

[10] So universal was the condemnation, that the Council of the British Association, in spite of the reception given to the paper and the proofs afforded of its correctness in principle, decided to omit all mention of the paper in their published *Transactions*. See Appendix, Note 3.

Henry, Edward Riley and Dr. Percy, the well-known chemists and metallurgists, and the publications of Robert Hunt of the Record Office of the School of Mines were also gone through. The ensuing experiments took the (as we now know) unfortunate path of trying to rid common British pig of its phosphorus. Bessemer says:—

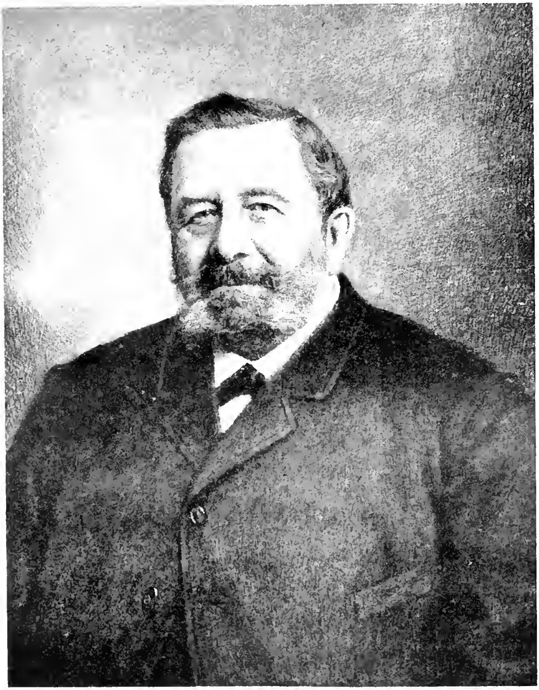
> "Apparatus was put up for the production of pure "hydrogen gas which was passed through the metal; as "also were carbonic acid, carburetted hydrogen, etc. Metal-"lic oxides and alkaline salts, and many other fluxes were "tried with little or no beneficial results, and the metal was "treated in various other ways. It is needless to follow the "continuous string of heart-breaking failures and disappoint-"ments, which were very costly and very laborious."

Bessemer now saw that the problem had to be tackled in a different way, namely, in the production of a phoshorus-free pig-iron, and, as a preliminary step, he sent to Sweden for some of the pure pig-irons used in Sheffield. Meanwhile, some eighteen months had been spent in useless experiments.[11] Bessemer says:—

> "Happily for me the end was nigh. The pure pig-"iron, which I had ordered from Sweden, arrived at last, "and no time was lost in converting it into pure, soft, "malleable iron, and also into steel of various degrees of "hardness. It was thus incontestably proved that with non-"phosphoric pig-iron my converting process was a perfect "success; and that with pig-iron that had cost me only £7 "per ton, delivered in London, we could, and did, produce "cast steel commercially worth £50 to £60 per ton by "simply forcing atmospheric air through it for the space "of fifteen to twenty minutes, wholly without the use of "manganese or spiegeleisen."

Now, neither in his autobiography nor in his account of the history of his process as prepared for the Joint International Meeting of the Iron and Steel Institute, the

[11] It is, probably, usually the case that theoretical research work is well ahead of its practical application. Bessemer's practical difficulties were not altogether justified by the actual state of technical knowledge at the time, had he or his advisers known where to look. See Appendix, Note 4.



GORAN FREDRIK GORANSSON.
1819–1900.

From a portrait supplied by his grandson, Mr. K. F. Göransson

American Institute of Mining Engineers and the Verein Deutscher Eisenhüttenleute, held in Allegheny City, Pa., 1890,[12] did Bessemer give any clue to the cause of this dramatic and swift change in his fortunes, but this part of the story is to be found in the following letter written to Professor Richard Åkermann, of Stockholm, by

GÖRAN FREDRIK GÖRANSSON.

"Dear Sir,

"In conformity to your friendly letter of the 2nd "instant, I will try to give you, in English, a short sketch of "the difficulties I had in commencing with the Bessemer "method and how I at last overcame them.

"My full name is Göran Fredrik Göransson. I bought "part of the Swedish Bessemer patent in June, 1857, "after a conversation with Sir (then Mr.) Henry Bessemer, "and on his promise that he would arrange the necessary "plant and send over an engineer for starting the manu-"facture in accordance with his patents.

"In the autumn he sent over his plant, namely, a "converter of his construction and a steam blast-engine "constructed by Messrs. Galloway & Co., of Manchester, "accompanied by an engineer for conducting the manu-"facture. We started in November, but we soon found "that the converter was impracticable, difficult to handle, "and giving no good result. We then constructed a fixed "converter (see *Pl. III.*) on the same principle as the small "fixed vessel which Mr. Bessemer used for his first experi-"ments at Baxter House, in London, but, according to "Mr. Bessemer's advice, with two rows of tuyeres, six in "each, the lower row at the bottom of the converter and the "other some inches above; but the result was not good. "Mr. Bessemer then advised us to augment the pressure of "the blast, and, to effect this, we took away the upper "tuyeres and used only the six below, each about five-eighths "of an inch in diameter, and we then sometimes succeeded, "particularly during the cold, dry, winter days, in getting "malleable ingots, but very irregular and generally full of "slag. The engineer, not knowing anything more than "myself, then left the works.

"We tried every possible means of augmenting the "pressure of the blast by reducing the diameter of the "tuyeres and using smaller charges, as we had reached the

[12] See *Trans. of A.I.M.E.*, vol. xix., p. 810.

"limit of the pressure which could be produced by the "blast-engine, but all with less and less success, and, on the "point of giving up the experiments, I resolved, in spite of "all advisers, to diminish the pressure and, instead, use a "larger quantity of air. For this purpose we put all the "twelve tuyeres in one line at the bottom of the converter, "and augmented the diameter of them to seven-eighths of an "inch to see what change they would produce. The result "was astonishing. The temperature of the fluid steel was "very much raised, the slag came up on the top beautifully, "the ingots turned out perfectly even in temper, free from "slag, and extremely malleable, more so than any iron made "on the old process. The first charge with the converter, "so arranged, was made on the 18th of July, 1858, and "from that date the Bessemer method can be regarded as "started.

"I sent fifteen tons of ingots to a firm in Sheffield and "fifteen tons to Messrs. Henry Bessemer & Co.'s works at "the same place, and went over to England in September "to have them tried. I found then that Mr. Bessemer had "not succeeded, but had to granulate in water the steel "he got from the converter and afterwards re-melt it in "crucibles. As such a process could not give any profit, "the friends who assisted him were losing all hope of "success, but they all came down to Sheffield to see my "ingots tried before they finally gave up this business. The "other Sheffield firm, thinking it their interest not to forward "theBessemer method, got the whole lot burnt at the wash-"welding, but the fifteen tons hammered and tilted at Messrs. "Henry Bessemer & Co's works turned out to full satis-"faction after having been tried for knives, scissors, razors, "other tools and plates.

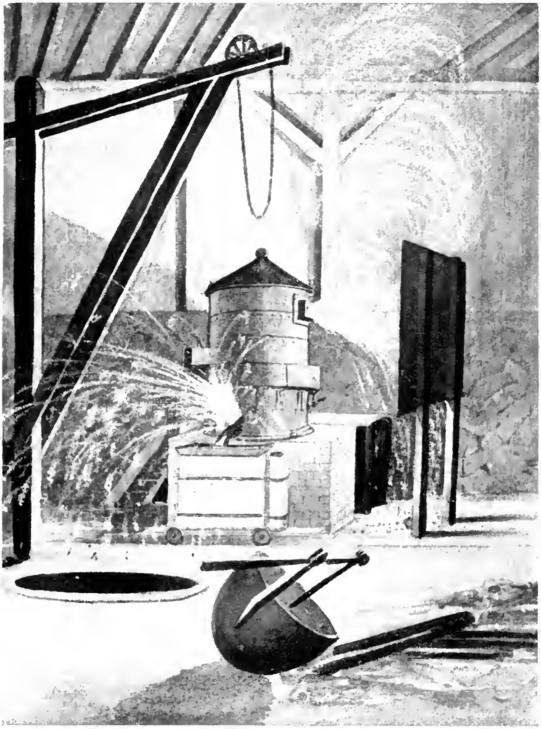
"The result of these trials has led to the further improve-"ment and to the present extension of the Bessemer process, "for which Mr. Bessemer and others have done much, but "Professor Viktor Eggertz's practical invention to find out the "percentage of carbon in the steel has overcome the greatest "difficulty then remaining.

"Faithfully yours,

"G. F. GÖRANSSON.

"Sandvikens Jernverks Aktie Bolag,
"Sandviken, November 6th, 1879."

The above letter is confirmed by reports from Mr. Grill, a Director of the Swedish Jernkontoret to his Board of



GÖRANSSON'S CONVERTER AT EDSKE, 1858.

Reproduced from a water-colour painting.

Directors, and published in the *Jernkontorets Annaler* of 1858 and 1859.

The conditions which favoured the success of Göransson were, in the first place, the great purity of the pig-iron. The resultant steel was therefore neither red-short nor cold-short. The pig-iron was also rich in manganese, so that the final steel contained this in sufficient quantity. The drawback of a low silicon was got over by taking the metal direct from the blast furnace and thus ensuring a hot blow.

The Swedish practice is to stop decarburization at the required point, and not to nearly completely decarburize and then recarburize. No manganese is added at the end of the blow, and Bessemer has made the most of this point in dealing with Mushet's claims that the success of the Bessemer process was dependent upon his patent for the addition of spiegel-iron at the end of the blow.

Bessemer, whilst ignoring his indebtedness to Göransson, yet—somewhat illogically—uses the circumstances of the success of Göransson's practice in Sweden and Austria as an argument for ignoring his indebtedness to Mushet's invention. I know of no better evidence of this than is contained in a letter written by Sir Henry Bessemer to myself, in the December of 1894, in connection with my Crewe lecture previously referred to. He was then in his 82nd year, and, in view of the historical importance of this letter, I have had the same reproduced in facsimile as here shown :—

Vide text of Sir Henry Bessemer's letter, over page.

I have looked up the Report of the United States Commissioners referred to in this letter, which fully confirms Sir Henry's statements. The iron employed was smelted by charcoal, was almost free from sulphur and phosphorus, and was rich in manganese. At the end of the blow, sufficient manganese was still present to prevent red-shortness, as, with manganese in excess in the pig-iron, free manganous-manganic oxide (MnO, MnO_2O_3) is reduced by iron carbide when no more free silica is present. The above Report draws attention to the success of the Swedish and Austrian mild steel plates thus made without added manganese, whilst the distrust of Bessemer steel plates in England was said to be due to the large amount of carbon unavoidably introduced by the use of spiegel-iron. This drawback was afterwards overcome in England and elsewhere—but not until the seventies—by the use of richer spiegels and by ferromanganese, which enabled the requisite amount of manganese to be added without unduly raising the carbon content.

The successful experiments with the pure Swedish pig-iron appear to have been made in the early part of 1858 at Bessemer's experimental works in St. Pancras, but, according to Göransson, the results were uncertain.

Bessemer, however, says in his autobiography :—

> "I announced the fact of my complete success to the "world, and held in my hands the most undeniable proofs "of the truth of my assertion ; but no one would believe it. "They remembered but too well the great expectations that "were excited two years previously by the first announce-"ment of my invention at Cheltenham, and were not again "to be disturbed by the cry of 'Wolf!' Thus it happened "that, after the hard battle I had fought for so many years, "I found myself as far as ever from the fruits of my labour, "for not a single ironmaster or steel manufacturer in Great "Britain could be induced to adopt the process."

Under these circumstances, Bessemer decided to build a steelworks himself in the heart of the steel industry, and so, in partnership with Longsdon, Allen and Messrs. Galloway, he started building operations in Sheffield, and these works were, in part, in operation at the time of

165, Denmark Hill,
Surrey.

December 8th 1894.

C. F. Lange Esqr

Dear Sir

I am not in possession of the diagrams relating to my late paper. but I have found a large one shewing in section the converter in the act of pouring its contents into the casting ladle also the hydraulic casting crane with man operating it, about 10 feet by 4 feet 6. painted on thick oil cloth, it is a little torn at one end. and stiffly curled up. you can nail it to a piece of board with small taks. at the torn end and then it will pass muster very well. — I also send a vertical

2)

section of the original upright fixed converter. used for my early experiments, but which has been much used in Sweden in Austria, and in Styria where Cutlery steel has for many years been made from their high class pig irons. and thousands of tons of this fine steel has been sent into this Country. all of which was produced by decarburizing to the desired extent, and not recarburized by Spiegelein nor by the application of Manganese in any form. excepting such manganese as the pig metal may have contained. this is interesting as shewing that the apparatus. and mode of working first proposed by me. has been proved to be a commercial fact. — a full confirmation of this fact is afforded by the Printed report of the United States Commission to the 1867 Universal Exhibition of Paris by Abraham S. Hewitt. see pages 60 and 61. of his report. —

I have this Morning forwarded these diagrams to the Secretary at Crewe. and have advised him by letter that I have done so. saying that you suggested my sending them direct to the Institute. as the time was short. —
I can only hope that these diagrams. may meet your wishes

Yours truly
Henry Bessemer.

P.S. A short time since I published a few Statistics of Bessemer Steel for private circulation among my friends. I shall be pleased if you will kindly accept the enclosed copy of the pamphlet. —

Göransson's visit to England in the autumn of 1858, as before related. Göransson found that Bessemer was making steel by granulating the converted charge by running it into water and then classifying according to the carbon percentage; the granulated metal being then remelted in crucibles. Göransson was able, during his visit, to put Bessemer into the way of his Swedish practice of blowing, after which, so far as I can learn, the granulating was abandoned—although not immediately—and the process thus established on a proper basis. There is a brief entry by W. D. Allen in an old diary for 1859: "First made steel direct." This was on June 18th, and thus fixes the date of the change.

The first year of working resulted in a loss of £729, and in 1859, the second year, the loss was £1,100. In 1860, simultaneously with the appearance of the converter in its present form, there was a profit of £900, and seven years later, the profit had risen to £28,000.

Bessemer says in his autobiography :—

> "Some idea may be formed of the importance of the "manufacture, and of how much the people of Sheffield lost "by their prejudice and incredulity, when I state the simple "fact that on the expiration of the fourteen years' term of "partnership of our Sheffield firm, the works, which had "been greatly increased from time to time entirely out of "revenue, were sold by private contract for exactly twenty-"four times the amount of the whole subscribed capital of "the firm, notwithstanding that we had divided in profits "during the partnership a sum equal to fifty-seven times the "gross capital. So that, by the mere commercial working of "the process, apart from the patent, each of the five partners "retired from the Sheffield works after fourteen years, having "made eighty-one times the amount of his subscribed "capital, or an average of nearly cent. per cent. every two "months—a result probably unprecedented in the annals of "commerce."

The principal product of the works in the beginning was tool steel. This sold at £42 a ton as against the ordinary Sheffield price of £50—£60 a ton, and was regularly supplied to important firms, including Messrs. Beyer, Peacock & Co. Bessemer had, by now, abandoned all attempts to purify common pig, and had turned his attention to the production of a special class of pure pig-iron from British ores, thereby laying the foundation of the great British hematite industry. In 1859 there were already two blast furnaces built at Ulverston for producing Bessemer hematite.

The selection of a sufficiently refractory converter lining, and the formulating of the first theoretical furnace charges so as to produce a British pig-iron suited to the Bessemer process, represented the solution of the last of the metallurgical difficulties that had faced Bessemer and his expert advisers, and which he has so graphically described. The best converter lining was found to be Sheffield ganister, which contained approximately 93% of silica, 4% alumina, 1 to 2% of oxide of iron, and, the remainder, the carbonates of potash, soda, and lime. In the puddling furnace the absence of silica in the slag enabled the phosphorus to form stable combinations with the iron oxides, but under no circumstances could this happen in the Bessemer converter, 40% of silica in the slag or even less sufficing to prevent this. Gruner,[13] of Paris, was the first to point this out, in the year 1857, a fact the importance of which for the Bessemer process could not be over-estimated. The rich hematites of Cumberland and Lancashire yielded a pig-iron sufficiently low in phosphorus and sulphur, and sufficiently high in silicon and carbon, to give a hot blow. The comparatively high silicon, and the comparatively low manganese, prevented, however, any working according to the Swedish method to which I have referred, and the metal had to be blown to nearly

[13] *Bulletin de l'Industrie Minerale*, T. II., p. 199.

ROBERT FORESTER MUSHET.

1811—1891.

From a portrait supplied by Mr. Samuel Osborn.

complete decarburization, and a sufficient quantity of spiegel added to give the metal the required composition. This practice also ensured the absence of red-shortness, even in the softest steels, by providing sufficient manganese to remove all oxygen from the bath and to counteract the effect of the sulphur. This addition of manganese, patented by Mushet, became a "world" practice, whereas the Swedish practice was, of necessity, a limited one.

ROBERT FORESTER MUSHET.

Robert F. Mushet was the son of David Mushet,[14] who, in his time, was the most important investigator into the science of iron- and steel-making in Great Britain. His no less renowned son was born at Coleford, in the Forest of Dean, in 1811. Now, whereas Bessemer, as beforesaid, makes no reference in his own accounts of his process to the value of Göransson's work in establishing the same, he devotes an important chapter in his autobiography to a detailed consideration of Mushet's claims. But, before dealing with this, I will give an account of Mushet's claims as written by himself in 1883, and for a copy of which I am indebted to my friend Mr. Samuel Osborn, of the Clyde Steel Works in Sheffield. He begins by explaining that having noticed that by alloying "burnt iron" with spiegel he could obtain a sound ingot, owing to the fact that the manganese combined with the oxygen of the iron, and thus restored it, he, thereupon, applied the same

[14] David Mushet was born at Dalkeith in 1772, and at the age of 19 entered the service of the Clyde Ironworks Company. He was a brilliant and hardworking investigator, but the value of his experiments was not understood by his employers. He later, in partnership with others, built the Calder Ironworks, but the enterprise ended in failure, and Mushet lost his money. Of great importance was his discovery of the blackband ironstone in 1800, which laid the foundation of the great Scotch blast-furnace industry as developed some thirty years later. Furthermore, his experiments for making crucible steel led directly to Heath's process.

to some pieces of Bessemer's pneumatised iron supplied to him by Thomas Brown, the then managing partner of the Ebbw Vale Iron Works, and made an ingot from the same that was sound and free from red-shortness. On September 22nd, 1856, or about five weeks after Bessemer's paper at Cheltenham, he proceeded to draw up a patent specification in which he claimed the addition of a triple metallic compound of iron, manganese, and carbon, to cast iron which had been decarburized by any process of forcing air through molten cast iron. By the bad advice of his friends at Ebbw Vale, this process was, however, coupled to that of Mr. S. G. Martien, to which I shall refer again. Mr. Brown's firm, later, made difficulties about the setting up of the necessary apparatus, and Mushet called to his aid a Mr. S. H. Blackwell, who found the money for this. The experiments were successful, and the complete specification was filed in March, 1857. The patent was thus arranged: the Ebbw Vale Co., one moiety interest; Blackwell, one quarter interest; and Mushet, one quarter interest. Prior to this, Bessemer had paid Mushet a visit at Coleford with the evident intention of obtaining his friendly co-operation, but Mushet felt himself tied to the Ebbw Vale Co. and nothing came of this. Pending the filing of the complete specification, the Ebbw Vale Co. were advised by some expert (wrongly) that the spiegel could not mix properly with the blown metal, and that Mushet's claim was worthless. Brown and Blackwell then omitted to pay the third year's stamp duty, and the process became public property. Mushet says:—

> "No doubt I might have sued them, but I had neither "health nor means of fighting a battle with them in a court "of law, besides which both Blackwell and Brown had been "personally kind friends to me, and both were then in "hopeless difficulties."

BESSEMER AND EBBW VALE.

To correctly understand the relations between Bessemer and Mushet, it is necessary to understand those between Bessemer and Ebbw Vale, as the former seem quite clearly to me to be the reflex of the latter, and cannot be separately considered. Bessemer relates that of the many persons who called on him after the Cheltenham paper with proposals for a licence, none was more energetic than Thomas Brown, of the Ebbw Vale Ironworks. Brown eventually made him an offer of £50,000 in cash for the patent rights in Great Britain for all his iron and steel inventions. This offer Bessemer refused, to the intense disappointment and chagrin of Brown. From then onwards, the attitude of Ebbw Vale to Bessemer was one of enmity. On September 15th, 1855, or a year previous to the Cheltenham paper, Joseph Gilbert Martien had taken out a patent for an improved method of making steel. The idea was to let the metal, tapped from a blast furnace or finery, flow through gutters which were perforated, in order to allow a stream of air or steam being forced through the metal, thus purifying it. Martien thought nothing about a decarburization or a generation of heat. This patent was bought by Ebbw Vale, and their furnace-manager, the well-known George Parry, tried to develop it. Parry tried treating 30 cwts. of molten pig-iron from a blast furnace by taking same into a reverberatory furnace, the bottom of which was perforated for an air-blast. A violent reaction was set up, the metal broke out, and the experiment was discontinued. Percy says, "lucky for Bessemer"; but this is not the fact. Others like Kelly[15] and Eck[16] had tried the same idea without knowing how to utilise it. Therefore, as

[15] *Engineer*, vol. lxxxi., p. 299.

[16] *Preussische Zeitschrift für Berg—Hütten und Salinenwesen*, Bd. XI.

Wedding says, Bessemer's deserts remain undiminished, for it is no real invention to have the idea of turning a known natural law to account without being in a position to indicate the right means for carrying it out.[17]

In 1857, Parry applied for a patent for decarburizing crude iron by blowing down upon it in a closed chamber without fuel instead of blowing upwards as in the Bessemer process. This was not completed. When Bessemer read his paper before the Institution of Civil Engineers in 1859, Brown was one of the speakers, and said that he had been sanguine of the success of the Bessemer process, and had spent £7,000 in endeavouring to carry it out (this without licence from Bessemer). He mentioned the difficulty which he experienced, amounting, indeed, to an impracticability in finding a completely refractory material for the furnace.

He criticised Bessemer's figures as to the cost of the materials, and said that with regard to waste there was, under the most favourable circumstances, a loss in the manufacture of nearly 40 per cent. of metal; and, on one occasion, his agent informed him that nothing but cinder remained.

In 1861, Parry took out a patent, the idea of which, evidently, was to get round the Bessemer process, as then practised, by applying the pneumatic process, not to pig-iron, but to a crude iron produced by melting scrap-iron with coke in a small blast furnace.

This patent, and the attempt to corner the use of manganese, caused Bessemer great anxiety. In 1864, Bessemer became aware, by accident, that negotiations were on foot to form the Ebbw Vale Ironworks and mines into a joint-stock company, with a capital of £2,000,000, a Mr. David Chadwick being the financial agent. Their

[17] Wedding, "Eisenhüttenkunde" iii., 2.

intention was to work the pneumatic process in conjunction with Parry's patent, and it would have cost Bessemer, according to his calculations, some £10,000 and a couple of years' struggle in the law courts to obtain his rights. Bessemer then threatened to apply for an injunction in the Court of Chancery to restrain the Company from using their obstructive patents (Martien & Parry) unless they immediately took out a licence to use his patents. In an interview with Mr. Joseph Robinson, the manager, and Mr. Abraham Darby, the chief proprietor of the Ebbw Vale Works, Bessemer drew attention to the fact that his invention would seriously reduce the value of a works arranged chiefly for iron manufacture, and that he was in a position to delay the floatation unless they agreed to his terms. A difficulty that arose was that they had not complete control over Parry's patent. Bessemer agreed to pay £5,000 to secure this, and also to deduct £25,000 from their first royalties in lieu of paying money for the purchase of all their patents. The deed of licence contained a full acknowledgment of the validity of Bessemer's patent. This agreement removed the last barrier to the quiet commercial progress of Bessemer's invention throughout Europe and America, and showed Bessemer in the light of a very astute business man. In fact, the Ebbw Vale royalties eventually amounted to between £50,000 and £60,000. This was Bessemer's last battle.

We can now, quite clearly, see the reasons for Bessemer's attitude towards Mushet. In his mind, Mushet was inseparably connected with the interests most hostile to him. The use of spiegel-iron was, at that time, just as essential to the one pneumatic process as to the other, and had Parry, with the wealth of the Ebbw Vale Company behind him, succeeded in getting round Bessemer's patent, he and Mushet could have compelled Bessemer to agree to any terms that they might have chosen to dictate. Mushet had identified himself not only with the wrong process but with the wrong persons. Under the circumstances, Bessemer's attitude towards him was but natural and reasonable, and Bessemer's subsequent treatment of him was, as I shall show, a not ungenerous one.

BESSEMER AND MUSHET.

In dealing with Mushet's claims, Bessemer first refers to the old knowledge of the use of spathose manganesian iron in steel-making, and to Heath's process of the use of carburet of manganese in crucible steel-making in common use in Sheffield. He draws attention to the fact that Mushet's patent of September 22nd, 1856, took a well-known compound, *i.e.*, "spiegeleisen," and that the invention which he proposed to improve by the same was not Bessemer's, but Martien's valueless patent. This was, in fact, a great error of judgment on Mushet's part, and one difficult to understand. Bessemer draws attention to the fact that his own patents covered the recarburization of the converted metal by pig-iron, but he clearly did not refer to a deoxidising agent thereby. Bessemer then refers to the unsuitability of spiegeleisen for mild steels, owing to its low manganese, and his efforts to introduce ferro-manganese.[18] Here he is upon safer ground, as, for the largest proportion of the world's Bessemer steel, ferro-manganese became a more important alloy than spiegel-iron. Bessemer gives a list of 24 patents relating to the use of manganese in steel, and covering a period from 1799 to 1856; he argues that if Mushet had invented an alloy suitable for the recarburization of mild steel and containing about 60% of manganese, 4% of carbon, and

[18] The contention here, apparently, is to set a limit to the scope of Mushet's services.

36% of iron, and had shown how to manufacture this commercially, he would have been entitled to a patent for his mode of producing it, and he denies his (Mushet's) right to patent the sole use of manganiferrous pig-iron.

In his earliest experiments, Bessemer avoided oxidation of the metal in the only way that he knew how to, namely, by practising a limited decarburization—that is, the practice later followed in Sweden and Austria. Some years before his death, Bessemer was able to show, through the finding of some of his old note-books, that by reading a description of Heath's invention in Ure's "Dictionary of Arts, Manufactures and Mines," he knew that red-shortness could be cured by carburet of manganese, and that he attempted to make carburet of manganese. These experiments, which were prior to Mushet's patents, led, after many delays and difficulties to the manufacture of ferro-manganese by Henderson, in Glasgow, at Bessemer's instigation. Meanwhile Mushet's patent had rendered Bessemer the service of drawing his attention to the fact that there existed a manganesian pig-iron—an alloy of carbon, manganese and iron. Spiegeleisen was not an absolute essential to the Bessemer process, although its ready availability removed, for a time, the difficulties that would otherwise have existed prior to the manufacture of other suitable manganiferrous compounds in the blast furnace.

Reviewing carefully all the evidence, it is impossible to deny the fact that Mushet did Bessemer a great and valuable service in drawing his attention to the utility to him of the existence of spiegeleisen, famed for steel-making on the Continent,[19] but it is equally impossible to

[19] Mushet had experimented with spiegel-iron obtained from Rhenish Prussia, and having a composition of 8·5% manganese, carbon 5·25%, and iron 86·25%, as far back as 1848; he does not refer to the fine exhibit of this material in the German section of the Great Exhibition at the Crystal Palace in 1851.

deny the fact that Mushet's patent, as far as it concerned the Bessemer process, was as bad as it could be. From the technical point of view, also, Bessemer's efforts to create the manufacture of a commercial high-percentage alloy of manganese were of far greater service to the steel industry than Mushet's patent, even had it been a good one.

Secure in the belief that Mushet's patent could not affect him, Bessemer used spiegel-iron for two and a half years (1857-60) after the date of that patent, when he was approached by a Mr. Clare, on behalf of Mushet, asking for an acknowledgment of the latter's rights, and offering a licence for the same for a nominal sum.[20] Bessemer, in reply, said that he did not acknowledge that Mushet had acquired the rights which he claimed, and invited legal proceedings, which, however, were never taken, nor did Bessemer ever hear again from Mushet. In December, 1866, Bessemer was visited by a young lady giving the name of Mushet. She said that grave misfortune had overtaken her father, and that without immediate help their home would be taken from them. She said: "They tell me that you use my father's invention, and are indebted to him for your success." Bessemer replied by saying that he used what her father had no right to claim, and that the only result of her father's patents was to point out to him some rights which he then already possessed, but of which he was not availing himself. This interview, however, ended, as might be expected by those who knew him, by Bessemer paying Mushet's debt—an amount of £377 odd. Bessemer's partners did not quite appreciate his generosity, thinking that capital would be made out of it. This did, in fact, follow, for, in the following year, a relative of Mushet approached Bessemer with

[20] This was shortly before the third year's stamp duty of £50 fell due, a matter to which I have already referred.

a request for an allowance for Mushet. Bessemer, to appease his ill-informed critics, then offered to pay him £300 a year, and this was done up to Mushet's death in 1891. Mushet, therefore, received over £7,000 from Bessemer, and I think that in view of all the facts as above detailed, it must be conceded that Mushet received ample, indeed generous, recompense from Bessemer. He was, indeed, much more fortunate than poor Heath whose invention, whilst enriching Sheffield, brought only disaster to himself.[21]

Mushet's name is, in reality, much more closely identified in the minds of men with the invention of the self-hardening tool steel of that name. Mushet discovered the self-hardening properties of tool steel, when alloyed with tungsten, in 1868, and three years later he was fortunate enough to get into touch with the late Mr. Samuel Osborn, of Sheffield, a man of great enterprise and business capacity, who quickly realised the significance of this invention.

From the beginning of his association with the firm of Samuel Osborn & Co., Mushet drew substantial royalties from the sale of his self-hardening tool steel, and I am informed that the rights of manufacture were subsequently commuted for a very considerable sum. The use of this tool steel quickly spread throughout the

[21] In 1845. Heath took out a patent for the manufacture of steel by the melting together of pig-iron and bar-iron, and then carburizing and purifying by carburet of manganese. This alloy he prepared by the deoxidization of black oxide of manganese by tar or other carbonaceous matter in crucibles in an air-furnace. He then suggested to the agent in Sheffield that it would be cheaper to make this part of the steel-melting itself. The information got revealed before he could cover this with a new patent, and then his licensees, on the plea that it was another process, refused to pay him royalties. Long and costly litigation followed, and Heath died a ruined and broken-hearted man. The House of Lords finally decided against Heath's heirs. Mushet has calculated that, up to that time, Heath's process had saved the Sheffield steelmakers £2,000,000.

world and gave an honourable significance to Mushet's name in every workshop.

Mushet's services were also acknowledged by the presentation to him of the Bessemer medal by the Iron and Steel Institute in 1876. It is strange that a similar honour was never conferred on Göransson. Mushet died in 1891, aged 80 years, having added still further distinction to the name he inherited. Of both lives—father's and son's—one could well quote "per ardua ad astra." His two sons continued in the employ of Messrs. Osborn up to a few years ago, when they retired from business.

PROGRESS OF THE BESSEMER PROCESS.

As I have already said, the merely experimental application of an old principle in a novel manner does not, of itself, constitute an invention, and Bessemer was, above all things, a brilliant inventor. His steel process required a vessel and equipment of entirely novel manufacture. The pear-shaped converter, with hydraulic tipping-gear and cranes and the ladle for vertical casting, were all designed by him, and are still unchanged in their essential particulars. It was Bessemer's fertility and ingenuity of invention, combined with great mechanical skill and infinite resource and perseverance, that enabled him to effect an industrial revolution, and to make his process the most successful method of producing steel in quantity that the world has known. In this, lies his claim to originality.

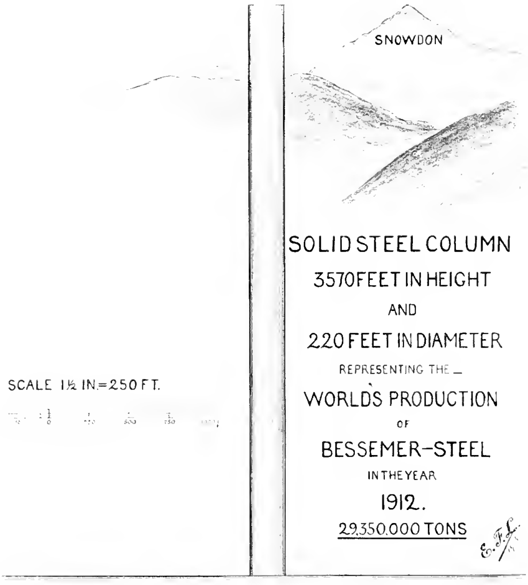
The new material of construction invaded all domains of industry. As may well be imagined, its use for rails instead of wrought iron was early considered as an attractive possibility.

In 1861 Crewe Station was laid with Bessemer steel rails rolled from ingots made at Sheffield, and the longevity of these rails soon demonstrated their relative cheapness. In 1862 Bessemer and Longsdon, in order to increase their available capital, decided to sell a portion of their patent rights. This they did to a syndicate of fifteen members, among whom were John Platt, of Oldham, and the Galloways of Manchester. For a sum of £50,000 the syndicate bought one-fourth share in the Bessemer process patents for the period of 1862-1884. This purchase proved enormously profitable to the syndicate. There is no doubt that the possession of this capital, and the strong supporting influence of the syndicate, greatly strengthened Bessemer's hands in his bold and decisive handling of his affairs with the Ebbw Vale Company two years later, to which I have already referred. In 1862 a Bessemer steel rail was put down at Camden by Mr. Ramsbottom, and it wore out seven iron rails, that is fourteen surfaces, before it was turned. In this same year the first Bessemer steel tube-plates were supplied to the Lancashire and Yorkshire Railway Company, and in the following year Bessemer steel was used for locomotive boilers on the London and North-Western Railway by Mr. Ramsbottom and Mr. Webb. The first boiler of this kind was kept in service until 1879. In that same year (1863) the same steel was extensively used for Lancashire boilers by Mr. Daniel Adamson, and it was also first used in shipbuilding.

A very important exhibit of Bessemer steel was made at the great International Exhibition in 1862. Among the articles shown were gun-forgings and projectiles, hydraulic cylinders, forged shafts, railway axles, engine and carriage tyres, bars and rods of all sizes, steel wire ropes, etc. It was this exhibit that directly led to the syndicate's purchase of an interest in the patents as above related. Large quantities of Bessemer steel rails were laid down by the Metropolitan Railway Co. in 1865, at which time the price was £17 a ton. The industry was now making great strides. In 1864 an American syndicate bought the United States patents of Bessemer, the celebrated Holley acting first as negotiator and later as engineering and metallurgical expert. By 1865 the process was well established on the Continent.

The discovery of a suitable basic lining for the Bessemer converter gave a great impetus to the process on the Continent, as the enormous deposits of phosphoric ore in Western Germany, Luxemburg, and Eastern France could now be used. I have already referred to the fact that dephosphorization is possible in the puddling process whilst the silica lining of the Bessemer converter made this impossible. Tunner, of Leoben, was the first to recommend a non-siliceous lining, but at that time none could be made. Daelen, in 1873, proposed lining the converter with iron oxide, but the practical difficulties were too great. In 1877 both Bell and Krupp took out patents for purifying pig-iron by oxide of iron. Snelus had already, in 1872, pointed the way to the true solution of the problem for the Bessemer process by patenting a basic lining, using a mixture of lime and magnesia with a little clay and oxide of iron, keeping the silica at a minimum, but it was not to the interest of the Company he was then working for to pursue the process. All difficulties in the way of producing a stable basic lining were, however, solved by Messrs. Thomas and Gilchrist in 1878, who discovered a method of making basic bricks from magnesian limestone, burnt at a very high temperature with aluminium silicate as a sintering material. The basic Bessemer—or Thomas—process, as it was called, had a swift development, and in 1889 the output was no less than 2,000,000 tons.

In his letter to me of December 8th, 1894, Sir Henry


SNOWDON
SOLID STEEL COLUMN
3570 FEET IN HEIGHT
AND
220 FEET IN DIAMETER
REPRESENTING THE
WORLD'S PRODUCTION
OF
BESSEMER-STEEL
IN THE YEAR
1912.
29,350,000 TONS
SCALE 1½ IN.=250 FT.

Bessemer refers to a booklet which he sent me, namely, a reprint from the *Engineering Review* of July 20th of that year, and entitled "A Brief Statistical Sketch of the Bessemer Steel Industry: Past and Present." In this article, of which he was the author, he attempts to give an idea of the world's production of Bessemer steel in 1892, which amounted to no less than 10,500,000 tons, by representing the same as a steel column 100ft. in diameter, the diameter of a large gasometer, and 6,684ft. high.

I am indebted to Mr. Harold Jeans for further statistics of Bessemer steel production since the year 1892, and find that the world's record output was achieved last year, 1912, with a total of 29,350,000 tons, or more than 2¾ times the amount that Bessemer, himself, found so impressive. This tonnage would lengthen the imaginary 100ft. diameter steel column from 6,684ft. to 18,682ft. in height.

In order to impress you with an idea of the magnitude of the above output in another way, I have prepared a diagram showing that this amount of steel would represent a column of 220ft. in diameter and 3,570ft. in height, or the height of Snowdon. (See *Pl. V.*)

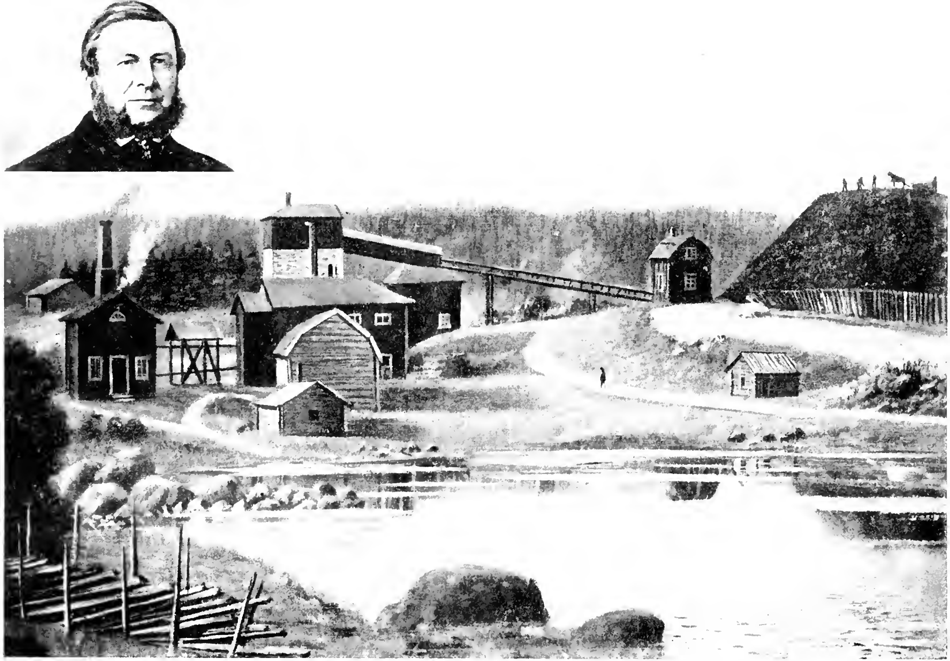
I feel, however, that I can with greater profit appeal to your imagination by mentioning some of the economic results of a process capable of producing cheap steel in such quantities. By enabling steel to be produced at a less cost than that of common iron, the cost of construction of railways has been so greatly lessened as to permit of their extension to the most distant regions, and the cost of transportation has been brought so low as to bring into the markets of the world perishable products which were formerly excluded from them.

These two causes have reduced the value of food products throughout the civilised world. Wages have risen all round whilst the price of food has dropped.[22] The working classes of to-day are enabled to earn and spend at least double the amount which was at their command at any previous age of the world. This result is, no doubt, almost wholly due to the economy in the means of production made by cheap steel,[23] and to the other inventions which have followed in its wake. These are material gains, but they are being followed by the slow but sure elevation of the great mass of society to a higher plane of intelligence and aspiration.

The first striking result of the cheapening in the cost of the production and transportation of food products was felt in Great Britain, which is now compelled to import at least two-thirds of its consumption. The competition of the American western wheat regions, with the products of India in the English markets, altered the whole conditions of agriculture in the British Isles. The profitable raising of wheat became practically impossible, and the farmers who had depended upon it could no longer pay the rents stipulated in their leases. A general reduction of rent became necessary, which, of course, reduced the income of the landlords. The aristocracy of Great Britain is a survival of previous conditions, depending for its existence upon the ownership of the land and the revenue derived from it. Hence a serious blow at, what may be termed, the privileged class of Great Britain was struck, of course unintentionally, by the invention of Bessemer. This has gone very far towards effecting a transfer of power from those who own the land to the commercial, manufacturing and industrial classes of the

[22] The comparison is with the era prior to the Bessemer invention. To-day, both wages and the cost of living are rising.

[23] The world's output of open-hearth steel is now much greater than that of Bessemer steel. See remarks on the future of Bessemer steel in the Appendix, Note 5.



EDSKE MASUGN,

DÄR BESSEMERS STÅLBEREDNINGS METHOD FÖRST GENOMFÖRDES DEN 18 JULI, 1858.

Reproduced from a sketch made in 1858. The inset is from a portrait of Mr. G. F. Goransson made in 1862. The title translated reads:—Edske Blast Furnace where Bessemer's method of making Steel was first accomplished, July 18th, 1858.

people. I think that it is doubtful whether any event of equal significance has occurred in modern times. We have, therefore, this curious result, that Sir Henry Bessemer has certainly been the great apostle of democracy, although I have never heard that he made this claim.

Göransson, to whose aid Bessemer owed so much of his first success, died at Sandviken, Sweden, in 1900, at the age of eighty-one. He was chairman of the Board of Directors and founder of the famous Sandvik iron and steel works. In 1865 he received the gold medal of the Swedish Jernkontor. He was a Knight Grand Cross of the Royal Wasa Order and a Knight of the Royal Order of the Polar Star. He was a man of the broadest culture and of a most lovable personality. On the occasion of the visit of the Iron and Steel Institute to Sweden in 1898, Mr. Göransson, although very infirm, welcomed the members, in an English speech, to the Sandvik works.

The greatest of this great trio of octogenarians, Sir Henry Bessemer, died, in 1898, at the age of 85, at his residence at Denmark Hill. He had been much affected by the loss of his wife in the previous year, after a happy union of more than sixty years. More fortunate than some great pioneer inventors, he reaped a full reward for his ingenuity and perseverance. In royalties alone he is said to have received altogether a sum considerably over a million sterling. He was less fortunate in "honorary" rewards, as these were insignificant in view of his great services to the industrial world. The Grand Cross of the Legion of Honour, which the Emperor Napoleon offered him in 1856, he was not permitted to accept, and the British Government only tardily acknowledged his services to the Inland Revenue Office in 1879. In 1858 the Institution of Civil Engineers awarded him the gold Telford medal. After the successful establishment of his process in Austria, he received the decoration of Knight Commander in the Order of Franz Joseph from the Emperor of that country. He was President of the Iron and Steel Institute, 1871-1873. In the latter year he founded his gift of the Bessemer Medal for the Institute. In 1879 he was made a Fellow of the Royal Society. He also received many other medals and diplomas. In 1880 he was presented with the freedom of the City of London. Of all the distinctions bestowed upon him, he appreciated as greatly as anything else the compliment paid him in the United States by no less than six cities being named after him. In 1872, Sir Henry retired from active business and sought relaxation in various hobbies, scientific and otherwise, and in the embellishment of his beautiful residence at Denmark Hill. There he built an observatory, and engaged on the construction of a large telescope designed by himself. There, also, he experimented with a solar furnace, built on a large scale, to produce high temperatures. To the last his mind remained clear and his body active. Those who enjoyed his friendship will remember his special charm of conversation and his facility for graphically explaining difficult subjects. In conclusion, I cannot do better than quote from an address by the late Hon. Abram S. Hewitt to the Iron and Steel Institute in 1890. After referring to the chief capital events of modern history, he says:—

> "I know it is very high praise to class the invention of Bessemer with these great achievements, but "I think that a careful survey of the situation will "lead us to the conclusion that no one of these has "been more potent in preparing the way for the "higher civilization that awaits the coming century "than the pneumatic process for the manufacture of "steel. . . . The name of Bessemer will therefore "be added to the honourable roll of men who have

"succeeded in spreading the gospel of "Peace on "earth and goodwill towards men," which our Divine "Master came on earth to teach and encourage."

ERNEST F. LANGE.

Fairholm, Willow Bank,
Manchester, 1913.

APPENDIX.

Note 1. Bessemer Velvet.

In connection with this subject, I received the following letter from Sir William Bailey :—

Sale Hall, Cheshire,
August 25th, 1913.

Dear Mr. Lange,

My finding of the Bessemer Velvet that I named to you was caused by dropping into conversation with the late Mr. Lamb, a well-known Manchester upholsterer, whose shop was in John Dalton Street. When Mr. Lamb was speaking to his manager about a contract that was in hand, he said, "Use Bessemer Velvet," and when the manager left the room, I found that in early life Mr. Lamb had been a friend of Sir Henry Bessemer's, and he had been using embossed velvet made by a process invented by Sir Henry. I was able to get 20 or 30 specimens from him. I sent one to the Bessemer Club in New York, which I understand is there now, and I also sent one to the late wife of Sir Charles Allen, the Managing Director of the Bessemer Steel Company at Sheffield; she was a niece of Sir Henry Bessemer. The following is an extract from the letter I sent to her :—

"Please receive herewith by pattern post a piece of embossed velvet with which Sir Henry Bessemer had to do when a young man.

"I am sorry I cannot give dates, but it must be between thirty and forty years ago that Mr. Pugin, the eminent architect, was engaged to design the draperies of the House of Lords, and he designed this pattern velvet, Sir Henry Bessemer designing and engraving the rollers for embossing it. I obtained it from Mr. Lamb, who knew Miss Bessemer (Sir Henry Bessemer's sister) well.

"He informed me that Miss Bessemer was also clever as a designer, and had to do with the embroideries of St. George's Chapel and Windsor Castle, Miss Bessemer being one of the first to introduce art needlework as an industry for ladies into London.

"Some very beautiful work was executed by Miss Bessemer's staff, and amongst it some of the tapestries at Chatsworth Hall. The Devonshire Banner, with the Devonshire Arms richly emblazoned in beautiful colours, now in the Hall, was executed under Miss Bessemer's superintendence by her staff of ladies."

Lady Allen sent half of this interesting specimen to her uncle, and in acknowledgment received the following characteristic letter from him:—

Denmark Hill, London, S.W.
March 31st, 1897.

My Dear Niece,

Allow me to thank you very much for the most interesting specimen of embossing in Utrecht velvet which you have been so kind to send me; it brings back old remembrances that will be for ever dear to me.

My sister was an artist of more than average ability in water-colour drawing, and excelled greatly in the art of embroidery in silk, and in due course was appointed embroideress to the Princess Victoria before she became Queen.

It is rather curious that I seemed born with an instinctive taste for designing patterns, and when I reflect on my natural aptitude for mechanical inventions, this old power of designing foliage and flowers, but more especially, grotesque ideal scroll work and foliage, it seems to me to have been a sort of faculty of inventing unseen forms in almost endless variety, and when I was only eighteen I designed for one year the principal Indian patterns for the great Indian silk merchants—Everingtons' of Ludgate Hill. It is a curious fact, in connection with your friend's letter, that I designed the patterns embroidered by my sister, in the draperies of the beautiful cradle of Her Gracious Majesty's first infant, at which early period I had the honour to be an exhibitor, together with my sister, at the Royal Academy, then held at Somerset House, in the Strand.

My sister had made a great number of flower paintings which she put together in a portfolio she had made, and on which she asked me to wite in bold printing letters, "Studies of Flowers from Nature, by Annie Bessemer." This little incident shaped my whole future life. I thought I would write the inscription in gold letters, and ordered two ounces of bronze powder (called also gold powder), but which is really only a beautiful fine brass, intrinsically worth eightpence per pound. I was charged fourteen shillings for my two ounces of brass powder, as a result that a material known and used in China and Japan for more than 1,000 years was still made by a roundabout hand-process, hence its great cost. I invented an elaborate series of self-acting machines and manufactured it successfully. My first order was obtained by my traveller from the Colebrookdale Iron Company for two pounds at eighty shillings per pound net. I kept the process a profound secret for about thirty-six years; it furnished me with the money necessary for pursuing my many patented inventions, and then the secret leaked out, prices went down and down, until I was selling the same article for which I had eighty shillings a pound as low as two shillings and ninepence, when I gave up the manufacture.

But I am letting my pen run away with me and forgetting all about Utrecht velvet. Between forty and fifty years ago I was exhibiting some specimens of castings from natural objects, cast in white metal, and which were coated by a thin film of copper deposited thereon from an acid solution of that metal. The exhibition was known as "Toblisses' Museum of Arts and Manufacture," which occupied the site of the present National Gallery in Trafalgar Square.

These specimens were seen and admired by Mr. Pratt, an upholsterer in Bond Street, and he sought me out, showing me a beautiful piece of velvet work of French manufacture; he proposed to produce a similar effect by embossing Utrecht velvet. He had tried the embossers of cotton velvet at Manchester, but they had utterly failed. This stubborn pile would not keep down, and the pattern was all gone in a few weeks.

I studied the question both from a chemical and mechanical point of view, made some experiments, and found that my plan was successful. The simple fact is that wool, like the hair of all animals, partakes of the property of horn, and is fusible by heat, but that high temperature is destructive if continued for more than a second of time, and my rollers would burn the whole fabric if worked too slowly. There were many details to work out, and when that was done I constructed the necessary machinery at my own cost and managed to get six shillings a yard for all the velvet I passed through the machine. The first work done by the machine was for the furnishing of a suite of rooms in Windsor Castle. With this good introduction the material became popular and fashionable, and, I may add, profitable. I increased the demand by lowering the price, and when it got down to one shilling per yard I sold the machinery to a manufacturer of Utrecht velvet at Banbury; the price eventually came down to twopence per yard, and then omnibusses and cabs were lined with it. My great difficulty was, that I could find no one capable of preparing the rolls, and had, as a last resource, to do it myself.

Your affectionate uncle,

HENRY BESSEMER.

This remarkable letter was written in his 85th year, a year before his death.

I am sorry that I cannot send you a copy of the letter Sir Henry Bessemer sent to me about the same time. I hope I have it in my papers.

Sir Henry's sister, Miss Bessemer, at one time taught the Princess Victoria, before she became Queen, art embroidery and blazonry at the old Kensington Palace. The Princess became an adept at the needlework of crests and banners and such like decorative work. If ever I find the last letter I will send you a copy.

Yours sincerely,

W. H. BAILEY.

Note 2. Bessemer and the Exhibition of 1851.

Bessemer exhibited some of his patented sugar-refining machinery and glass-polishing apparatus at the first International Exhibition at the Crystal Palace in 1851. At this Exhibition the most important exhibits of steel were those of Krupp, and from Siegen came a fine exhibit of Spiegeleisen, the potential possibilities of which were little realised at the time by those who saw it, and who were later to be connected with the Bessemer process.

Note 3. The British Association and Bessemer and other authors of Papers.

In the *Report of the Transactions of the British Association* for 1856 only the title and the author of the paper on "The Manufacture of Malleable Iron and Steel without Fuel" is given, a no better treatment than was later given to De Rougemont, the great hoaxer. The 1898 Report contains the following:—Section E, page 943. "Twenty-eight years in Central Australia," by Louis De Rougemont, and Section H, page 1015, "On the Natives of North-West Australia," by ditto. With regard to this matter, Sir William H. Bailey wrote to me on August 25th, as follows:—

> The difficulties that assail pioneers can be further exemplified by a mention of those that had to be surmounted by the greatest discoverer of the nineteenth century—Dr. Joule.
>
> He wrote a paper, making public the Laws of the Conservation of Energy and the Mechanical Equivalent of Heat, for the British Association Meeting, in 1844. I believe that this was the Cork Meeting, as my late friend, Mr. Denny Lane, who had to do with it, was then manager of the Cork Gas Company—he later became the Chairman of Directors. Mr. Denny Lane was requested by a friend of Joule's to get together an audience, as it looked as though Joule's paper would be read in an empty room, for even those friendly to him took no interest in his discovery, not understanding the same. One of the officials also sought Mr. Lane's help in getting together a few listeners.
>
> Mr. Lane, in relating this to me, some time later, said, "I was one of five, and we listened for politeness' sake to Joule, and not one of us was any the wiser when he had finished his paper, as we could not understand it."
>
> I believe that Joule mentioned his experience to his brother baptist who was honorary organist at St. Peter's Church, and where Mr. (afterwards Sir) Thomas Sowler, the Editor of the "Manchester Courier," was Churchwarden. Through Mr. Sowler's kind influence, a number of young men connected with the Sunday School of St. Peter's formed another audience somewhere near St. James's Square, I think in Back King Street, and this obtained for the discoverer a paragraph in the "Manchester Courier." Afterwards we know that it gradually dawned upon the scientific societies in Europe that this son of a Salford brewer had discovered the greatest generalisation of the Laws of Force and Energy. It is not to be wondered at that a new discovery has a difficulty in getting an audience, for all discoveries of great

moment have generally been made by amateurs, and it is a long time before the men who are proclaiming a new truth in the wilderness obtain scholars, students, and followers.

W. H. B.

Note 4. Metallurgical Science at the time of Bessemer's invention.

Fourcroy, the celebrated French chemist and Minister of Education in France, on the threshold of the 19th century, expressed the following prophetic sentiment:—"L'art de fer, dans ses divers degrés de perfectionnement, marque exactement le progrés de toute civilisation."

Napoleon, who had a profound belief in science as an aid to power, called to the service of the State France's most distinguished scientists. Under Fourcroy were gathered the mathematicians La Place, Monge, Carnot, and the chemists Berthollet, Chaptal and Guyton de Morveau.

The first important works on the metallurgy of iron were written by Hassenfratz and Héron de Villefosse to Napoleon's orders. Lavoisier had already investigated the processes of oxidation and reduction, and had thereby explained the majority of metallurgical processes.

The mysteries of the different forms of iron had been investigated by Berthollet, Vandermonde and Monge. Claude Louis Berthollet, elected a member of the Manchester Literary and Philosophical Society in 1790, was perhaps the first to point out that German steel made from spathic ores always contained manganese. He accompanied the French Expedition to Egypt, and was, later, given the title of Count.

Napoleon Bonaparte had already, as first Consul, in 1800, proclaimed to the French that "Gold and Iron" were necessary to command peace; hence his great efforts to promote metallurgical science and industry on the

Continent. His attempt to close all markets to England —the so-called "Continental blockade" of 1806—led, eventually, to Russia leaving the coalition in 1810 and joining England, thus setting in motion a train of events that led ultimately to Napoleon's downfall. This blockade did the most harm to the Continent, as England, owing to her insular position, had got a great start in the iron and steel and allied engineering industries, and was really the teacher of all Europe. The blockade merely served to delay similar industrial developments on the Continent; thus it was that Napoleon's political intrigues reacted on himself, and that England, in the end, achieved that "Gold and Iron" necessary for victory and subsequent peace, but not as Napoleon had planned.

To David Mushet's services to Great Britain in respect of metallurgical research, I have already alluded. In 1810 the great Swedish chemist, Berzelius, determined the exact composition of the oxides of iron; along with Thompson, Wollaston, and Davy, he had done most to advance Dalton's views, first brought before this Society in 1803. In 1821 Karl Karsten was given the highest official position related to mining in the Ministry of the Prussian King, Frederick William, under his well-known Minister, Von Stein. He can be said to have been the first to have put the Silesian iron industry on a sound footing, and in addition to an immense activity and practical knowledge, he was the most important author of his period on the metallurgy of iron and steel. The three editions of his great work on "Eisenhüttenkunde," published in 1816, 1827 and 1841 respectively, give the most complete survey of the progress of iron and steel manufacture over that period. The researches of Berthollet and Stünckel on spathic ores appears to have led that indefatigable Lancashire ironmaster, John Wilkinson, into taking out a patent in 1808 for adding manganese, or manganiferous ores, to iron ores for the production of iron to imitate the Continental steels made from spathic ores. Klaproth's accurate analyses of manganiferous iron ores were already then available. Karsten carried still further Mushet's investigations into the carbon contents of various steels and irons, and he, still further, showed the effect of sulphur in producing red-shortness, and of phosphorus in producing cold-shortness.

The investigations of Berthier, Berzelius and Karsten on blast furnace and refinery slags, and the reduction of iron by carbon, threw important light on the working and control of furnace charges. The alloys of iron and steel were investigated by Stodart, Faraday, Berzelius, Stromeyer and others.

The analyses of iron and steel and other metallurgical researches by Liebig, Gay-Lussac, Davy, Wöhler, Wilson, Thompson and others were made known by the publications of Hartmann. In 1851, appeared Rammelsberg's handbook on the chemistry of metallurgy, and, in 1855, the handbook of Bruno Kerl, and, in the same year, the work of Truran in England. In 1857, Gruner pointed out that the acid slag of the Bessemer converter barred the removal of phosphorus. In fact, the period of 1851 to 1860 was very rich in metallurgical literature, mostly of German and Swedish origin. It is not possible to do more here than thus give a brief glance over the wide range of technical knowledge appertaining to iron and steel that existed prior to Bessemer's discovery, yet it is sufficient to illustrate the old lack of co-operation between practice and theory, a feature not unknown to-day, as those who, like myself, have occasionally to investigate new processes have had opportunity of observing. One cannot, indeed, help but reflect, on reading about Bessemer's long, fruitless and costly experiments during the period of 1856-1858, how infinitely useful would have been to him and his advisers a polytechnic, or sort of technical clearing-house, of information bearing upon the problems with which they were contending.

A new era, however, was to dawn before the advance of technical education, and the widespread publicity given to theoretical and experimental research work furnished to a would-be inventor a ready knowledge of what had been already accomplished and, to some extent, of what remained to be done.

The union of theory and practice is a modern development, for the fact remains that most of the great inventions of the past owed their creation to men whose qualifications were mainly practical, and who were guided by instinct and intuition rather than by scientific knowledge. Bessemer was no exception to this. He, himself, is reported to have said that had he been a metallurgist, he would not have come to the idea of his steel process. This saying expressed a half truth only, for the romance of industry, as of art, abounds in examples of genius bursting its bonds and following an apparently irresistible impulse towards new and unexpected achievements.

It has, however, become more and more recognised that Science is the indispensable hand-maiden of Industry, and that the most rapid industrial developments of recent years have been found where the highest technical training has been employed.

Technical science is, to-day, the greatest factor in industrial progress, and is destined to become the greatest leveller of nations.

Note 5. The Future of Bessemer Steel.

Whilst it is true that in 1912 more Bessemer steel was produced than in any other previous year, it is also the case that the open-hearth process now takes, by far, the premier position as regards output.

In 1907, the world's outputs of Bessemer and open-hearth steel were about equal. In 1908, the Bessemer output was about 1½ million tons less than that of the open-hearth output; whilst, in 1912, the Bessemer output (29,350,000 tons) was about 11½ million tons less than that of the open-hearth output (41,022,000 tons). It will be seen, therefore, that the Bessemer process is fast losing ground in comparison with its newer rival. This is due to two main causes. In the first place, it is due to the gradual exhaustion of suitable ores for the Bessemer process, and, in the second place, to the great development of the open-hearth process, due to the abundance of suitable material for the same and its advantages in respect of the utilization of the world's ever-increasing supply of scrap iron and steel.

The introduction of the electric furnace for steel-refining, the most modern of metallurgical developments, appears likely to give renewed vitality to the Bessemer process, experiments which have recently been made in the United States, Germany and French Lorraine, in giving Bessemer steel a short final treatment in the electric furnace, having proved most successful.

ERNEST F. LANGE.

A B

TYLODENDRON COWARDII

Manchester Memoirs, Vol. lvii. (1913), *No.* **18.**

XVIII. A Tylodendron-like Fossil.

By F. E. WEISS, D.Sc., F.L.S.,

President of the Society.

(*Read April 22nd, 1913. Received for publication September 29th, 1913.*)

At a meeting of the Manchester Literary and Philosophical Society held on December 10th, 1912, Mr. T. A. Coward exhibited a silicified specimen which appeared to be a plant remain of some nature, but the origin of which was somewhat doubtful. It was brought to his father by a labourer, who stated that he had picked it up in a brickfield in the neighbourhood of Altrincham, Cheshire. It seems unlikely, however, from the nature of its preservation, and also from the structure of the plant remain as described below, that the locality in which it was found was the place in which it occurred in nature, though it is equally puzzling to know how it got there.

The specimen has now been more carefully studied, and I can give the following account of it.

In form it is barrel-shaped (see *Pl. I.*), but somewhat elliptical in section. It measures 7 cm. in height, and at its widest point it has a diameter of 6·3 cm., while it tapers away at the top and bottom to 4·5 cm.

It shows more or less clearly on the outside narrow, rhomboidal markings, which are arranged in a steep spiral, and each of these areolae is marked by a linear depression extending about one-third of the length of the space.

These markings and the general shape of the fossil lead to the identification of the fossil with Tylodendron, a genus instituted in 1869 by Weiss,[1] who considered it to be a conifer, the slitlike depression being regarded by him as a resin-canal, such as occur in most of our recent conifers. Potonié,[2] however, came to the conclusion that Tylodendron is not the complete tissue of a fossil plant, but only the pith-cast of a coniferous tree, possibly closely related to Araucaria. The areolae he correctly identified with the markings of the primary medullary rays where they join the pith, while the depressed grooves around them indicate the positions of the primary wood. The narrow slit in the cushion-like area represents the position of the leaf-trace not quite median to the cushion, but slightly on one side.

Potonié has shown by the comparison of Tylodendron with the pith cast of *Araucaria brasiliana* that the marking of the two is in close agreement, and also that the occasional barrel-like swellings appear to represent the increase in thickness of the medullary region at the points where the whorls of branches are given off. In one specimen of Tylodendron, at all events, described by Potonié, branch scars and remains of branches have been met with, and also portions of the surrounding woody cylinder. For the most part, however, the specimens of Tylodendron are mere pith casts. In some cases, however, the pith has been silicified, so that its structure and that of the adjacent woody tissues have been able to be examined by Potonié. These specimens have shown that the wood possessed the structure generally described for Araucarioxylon, and particularly with the species *A. Rhodeanum*.

[1] Weiss, Ch. E. "Fossile Flora der jüngsten Steinkohlen-formation und der Rothliegenden im Saar-Rhein-Gebiet." Bonn, 1869.

[2] Potonié, H. "Uber die fossile Pflanzengattung Tylodendron," *Jahrbuch der Königl. preuss. Geologischen Landesanstalt*, 1887.

The size and general external appearance of the fossil exhibited by Mr. Coward is in close agreement with one of the periodic enlargements of Tylodendron. There are, however, certain internal features in which this plant differs from those described by Potonié and which warrant a careful description of its structure.

A transverse section through our specimen shows that except for a few longitudinal fissures the tissues are fairly well preserved throughout the specimen, though the tissues do not allow of the most detailed examination. Almost the entire mass consists of pith or medulla, but along the circumference there are disconnected masses of lignified tissue consisting of the woody tissues of the stem.

The pith is seen to be composed entirely of thin-walled parenchyma similar to that described by Potonié. The cells are polygonal, more or less isodiametrical in shape, sometimes, however, slightly longer in a horizontal direction, while towards the outside, where they approach the protoxylem, the long axis of the cells is parallel to that of the woody elements. The size of the parenchyma cells varies from 8-11μ in their shorter diameter and from 15 to 16μ in their longer diameter. The outer portions of the pith have very numerous secretory canals with dark brown contents, very like those found in the Medullosae, in Cycads, and also in the pith of Poroxylon. These have not been observed or described, as far as I know, in any other specimens of Tylodendron showing structure. Each of these ducts is surrounded by eight or more cells, often slightly radiate in their arrangement, of large parenchymatous type, similar to those of the rest of the pith (see *Pl. II., Fig.* 1). The canals are always filled to some extent by a darker coloured mass, probably the remains of whatever secretion they contained. In longitudinal section they are seen as long and continuous passages passing vertically between the cells of the pith, and in this view, too, they resemble the mucilage canal of the Cycadales. It will be seen, however, that in our specimen of Tylodendron the duct has usually no specialised secretory cells of smaller size than the ordinary pith cells. It is, therefore, of a less highly organised or specialised nature. In some cases, as illustrated by *Pl. II.*, *Fig.* 7, some of the surrounding cells may show that a division has taken place, though this is not the usual condition. In one case (*Pl. II.*, *Fig.* 4) repeated division has taken to form a very specialised secretory and surrounding layer.

These secretory passages form a very striking feature of the pith, and are certainly very distinctive of the Tylodendron under consideration. They are not found throughout the pith but are numerous in the outer layers to the depth of about 1 cm. from the periphery. Their course, as seen in longitudinal sections, is vertical, without bending about as is sometimes the case with similar ducts. I have not seen them passing out through the foliar gaps, nor do they seem to have any connection with the leaf-traces. Running vertically as they do, they do not show clearly any anastomosing, but sometimes two may be seen running closely side by side; and from such transverse appearances as are shown in *Pl. II.*, *Figs.* 2 & 3, one may conclude that at times they unite after running side by side for some distance. In no case have secretory passages of any kind been found among the woody tissues of the plant.

The pith cells are all parenchymatous in character, and, as is seen most clearly in longitudinal sections, slightly drawn out in a horizontal direction, except at the outside, where they join on to the vascular tissue. Here their long axis is parallel to that of the adjoining tracheids.

In most parts of the section the walls appear entirely without marking, but in some regions the cell walls are impregnated with reddish brown colouring matter, and then the walls are marked with distinct oval pits. There is no indication in the longitudinal sections of the existence of any transverse diaphragms such as are shown on the specimens of Tylodendron from Prince Edward's Island, described recently by Miss Holden.[3]

The lignified tissues are found in the depressed regions forming the boundary of the lozenge-shaped areas. In places where the rhomboidal marking is indistinct on the outside, this is due to a larger amount of the wood having been preserved, so that the projecting masses of the pith corresponding to the foliar gaps are not so clearly shown. The short depression running up about a third of the length of some of the areolae, and which has been identified by Potonié as the leaf-trace, is also composed of vascular tissue, and there is no doubt that Potonié has correctly interpreted this feature of the Tylodendron cast.

Unfortunately, the state of preservation of our specimen is not sufficiently good to make out all the details of structure of the wood, but some features of interest can be definitely ascertained. It will be seen, as shown in *Pl II., Fig.* 6, that small groups of tracheids are found on the inside of the main mass of secondary xylem, and usually separated from it by one or more rows of parenchyma cells. These groups are of small size. That represented in *Pl. II., Fig.* 6, is one of the largest and the best preserved, but they sometimes seem reduced to a single tracheid. They are often surrounded by parenchyma cells, which are drawn out radially from the group so as to present a stellate appearance, not unlike the ground

[3] Holden, Ruth. "Some Fossil Plants from Eastern Canada." *Annals of Botany*, vol. xxvii., 1913.

tissue surrounding the primary wood of *Pitys antiqua*.[4] But these isolated patches are never as large as those of *Pitys antiqua*, and are sometimes almost continuous with the main woody cylinder. In these small groups it has not been possible to note much difference in the size of the tracheids, and we cannot therefore determine whether they had a mesarch or endarch structure, though the particular group figured may suggest a mesarch arrangement. From longitudinal sections it would appear that the primary wood is somewhat sinuous in its course—sometimes bending away from the secondary wood towards the pith, sometimes joining close up to the secondary tracheids. This would to some extent explain the irregularity of these isolated groups of tracheids. If these groups represent the primary wood, we may, I think, conclude that their feeble development represents the transition to a different type of vascular development, and nearer to that found in recent Gymnosperms. The occurrence would, indeed, be an analogous case to that described by Zalessky[5] in *Mesopitys Tchihatcheffi*, though occurring probably in a different series of forms. Similar isolated groups of tracheids are figured by Wieland in *Cycadoidea Wielandi*, where sometimes two groups may be found on the same radius. The longitudinal section represented in *Pl. II., Fig.* 7, may possibly represent two such radial groups, or both isolated tracheids may belong to a larger group, cut tangentially.

The main xylem masses commence with small tracheids, but only in one case could a distinct spiral vessel be observed, otherwise both the isolated and the innermost tracheids show scalariform markings. The

[4] Scott, D. H. "Studies in Fossil Botany," 1909. Vol. ii., p. 516.

[5] Zalessky, M.D. "Étude sur l'anatomie du *Dadoxylon Tchihatcheffi*." *Memoires du comité geologique*, St. Petersbourg, 1911.

tracheids nearer the outside are pitted. There seem also to be transition stages between the two. In some of the outermost scalariform tracheids the pits are wide and oblong (*Pl. II., Fig. 8a*), and in some of the next tracheids, which have two rows of pits, one notices here and there cases of "fusing" of two lateral pits (*Pl. II., Fig. 8b*), both of which cases have been recorded for the Araucarineae by Boyd Thompson.[6] Where the rows of pits occur they are usually alternate, and being somewhat crowded become slightly polygonal in shape. (*Pl. II., Fig. 8c.*) The outermost tracheids have usually only one row of pits. (*Pl. II., Fig. 8d.*) The state of preservation is not sufficiently good to show in the longitudinal sections the nature of the outer pore of the bordered pit. But that the pits were bordered can be inferred from certain of the transverse sections where a distinct lens-shaped thickening of the radial walls is recognisable, and in one or two cases traces of the middle lamella were observed. The comparative sizes of tracheids and pits are as follows: Scalariform tracheids, ·02 to ·025 mm.; pitted tracheids, ·025 mm., somewhat narrower in the tangential diameter. Where one row of pits occur, the latter are slightly flattened above and below by the adjacent pits, and have a horizontal diameter of ·017 mm. and a vertical diameter of ·015 mm. Where two rows of pits occur, each has a horizontal diameter of about ·012 mm. and a vertical diameter of ·01 mm.

The medullary rays are numerous. They are generally one cell in thickness, and vary in height from one to about six rows of cells. It is impossible from the state of preservation to say whether there are any ray tracheids or not. The cells of the medullary ray, as far as one can tell, are all of one kind. They are horizontally drawn out so as to be in contact with about five or six tracheids.

[6] Thompson, R. B. "Anatomy and Affinities of the Arancarineae." *Phil. Trans.*, vol. 204, 1913, p. 18.

The leaf-traces which are given off from the base of the foliar gaps are of fair size and are not quite median in their insertion, but seem to be attached for a short distance to one side of the gap. They run vertically for some distance, about 1/3 of the length of the foliar gap, and can be seen to consist of two distinct branches separated by parenchymatous tissue. Towards the base these two strands join and then fuse laterally with the xylem mass of the stem. Each branch has a small group of central tracheids, sometimes separated from the regularly arranged rows of secondary tracheids. In transverse sections it was not possible to determine the position of the protoxylem owing to the slight differentiation in the size of the tracheids, but a good longitudinal section indicated an endarch arrangement. After running vertically upwards for some distance, the leaf-trace bundles bend sharply outwards, and can no longer be followed in the specimen, which is limited to the pith and the adjacent tissues.

From the foregoing description of our specimen it will be seen that it agrees, in the main, with other Tylodendra, but it is not possible to be quite definite as to the systematic position of the plant to which it belonged.

As stated at the commencement of this paper, Potonié definitely identified the Tylodendron remains originally described by Weiss and re-examined by himself as pith casts belonging to the stems of *Araucarioxylon Rhodeanum*, which itself was probably the stem of the genus Walchia of Sternberg. At all events Tylodendron was the pith of an Araucarian tree. Seward[7] found somewhat similar pith casts, but devoid of periodic dwellings, which he correlated with the genus *Voltzia heterophylla*, which is

[7] Seward, A. C. "Tylodendron and Voltzia." *Geol. Magazine*, Decade iii., vol. vii., No. 311, May, 1890.

generally regarded as a member of the Taxodineae, but has also been considered as possibly Araucarian.

The Tylodendron remains from Prince Edward Island in Canada, examined by Miss Holden,[8] differ from the one described in this communication in the possession of a discoid pith being similar to that of the Cordaitales, but the detailed structure of the wood is of Araucarian type, so that Miss Holden leaves their affinities undetermined. She considers them not closely related to the Cordaitales, and there seems to her "to be no sufficient grounds for relating them to any group of living conifers."

The specimen described in the present communication differs in the structure of its pith, both from the Tylodendron described by Potonié and from the Canadian forms, by the possession of numerous secretory passages. The presence of these canals, resembling so closely those of many of the members of the Cycadales, inclined me at first to the view that this silicified inner portion of the stem might have belonged to a member of that group. On the other hand the structure of the secondary wood is not Cycadian in nature but more of the Araucarian type, the pits being in one or two rows, and not occupying the whole width of the tracheid, a point on which Gothan lays some stress.[9]

A reconciliation between the Cycadian or Cordaitalean nature of the pith and the Araucarian nature of the wood is now possible, since Thompson has described mucilage ducts in the medulla of the cone of *Araucaria imbricata*.[10]

[8] *loco. cit.*

[9] Gothan, W. "Uber die Wandlungen der Hoftüpfelung bei den Gymnospermen im Laufe der Geologischen Epochen." *Sitzungsberichte, der Ges. naturf. Freunde*. Berlin, 1907.

[10] Thompson, R. B. "On the Comparative Anatomy and Affinities of the Araucarineae." *Phil. Trans*, Series B., vol. 204, 1913.

As Thompson rightly considers that the cone axis often preserves ancestral characters, he believes that the mucilage ducts in the medulla of the latter may represent the "retention of a primitive form of organisation," and it may be that ancestral Araucarineae possessed mucilage ducts in the medulla of their stem. The presence of secretory canals, possibly mucilage ducts in our Tylodendron, might therefore not militate against its identification as a member of the Araucarineae. It must be pointed out, however, that the mucilage ducts of our Tylodendron are far simpler in structure than that figured by Thompson for *Araucaria Bidwellii.* The latter is surrounded by a large number of cells, and there has evidently been a considerable meristematic activity of the cells surrounding the canal. In the Tylodendron the canal is normally surrounded by 8 to 10 large parenchymatous cells of the ordinary type, making up the medulla, and only in one or two cases were new divisions visible in the latter. The canals were therefore of a more primitive type than those of the recent Araucarias described by Thompson. This is, of course, what one might expect in an extinct form. On the other hand the peculiar secretory canal shown in *Pl. II., Fig.* 4, with the very numerous divisions of the surrounding cells, shows that the plant already possessed the power of forming a much more elaborate duct not unlike the large mucilage canal figured by Thompson for recent Araucarias. There are, it is true, other differences between the secretory canals of our Tylodendron and the mucilage of recent Araucarias. Those of Tylodendron were never observed to pass out beyond the pith, but it is possible that they may do so occasionally, and though they do not regularly anastomose, occasional fusion of two more or less parallel canals seems to have taken place.

The structure of the secondary wood, consisting as it does largely of tracheids, with one or two rows of pits, points to Araucarian affinity of our fossil, though the considerable number of scalariform tracheids which precede the pitted elements seems to indicate a primitive form of that group. As Thompson[11] has now confirmed the old view of the derivation of the Araucarineae from the Cordaitalean stock, we may expect our fossil to show some features of the latter. The secretory canals referred to above may be considered in that light as well as the large number of scalariform elements. Moreover, we must remember that even among the Cordaitales we now know of members with bordered pits in one or two rows; for this is one of the diagnostic characters of *Mesoxylon poroxyloides* described by Scott.[12]

The remains of groups of tracheids possibly representing primary centripetal xylem may also be regarded as of a primitive nature, and, ill-defined as these are, they may indicate, as Zalessky[13] has said of the same phenomenon in *Dadoxylon (Mesopitys) Tchihatcheffi*, "la marche probable des modifications de la structure mesarche du bois primaire des anciens types gymnospermes dans la direction de celles qui est propre aux espèces actuellement vivantes du meme groupe."

We obtain no help towards the identification or classification of our fossil plant from the fact that it possesses foliar gaps and double leaf-traces. These characters it shares both with the Araucarineae[14] and the Cordaitales.

[11] *Loc. cit.*, p. 42.

[12] Scott, D. H. "The Structure of *Mesoxylon Lomaxii* and *M. poroxyloides*." *Annals of Botany*, vol. xxvi., Oct. 1912.

[13] Zalessky, M. D. "Étude sur l'anatomie du *Dadoxylon Tchihatcheffi*, Goeppert. *Memoires au Comité geologique St. Petersbourg*, 1911, p. 21.

[14] Thompson, *loc. cit.*

We are thus left with the conclusion that our Tylodendron-like remains belonged to a plant showing certain Araucarian characters (secondary wood) while some of its structures (scalariform tracheids, secretory passages, and possibly groups of centripetal wood) indicate it to be more primitive than the recent Araucareae, and in these characters it was more in agreement with some of the Cordaitales.

This conclusion is of interest when we remember that some of the Tylodendra described by Miss Holden possessed discoid pith. Probably various members of the Cordaitales and the Araucarineae, as well as plants intermediate between these two groups, may have given rise to Tylodendroid remains, and thus their investigation may throw some light on the relationship of these two groups of plants.

Other groups of Gymnosperms, too, may have possessed a pith which, when preserved, has presented a similar appearance. Thus, the pith casts described by Seward belonged in all probability to Voltzia, which is considered to be one of the Taxodineae

Until the specimen described in this communication can be more definitely identified, it might for convenience be referred to as *Tylodendron Cowardii.* The remains of the fossil and the sections cut from it are now in the Museum at the University of Manchester, Mr. Coward having presented the specimen to that institution.

EXPLANATION OF PLATE I.

Tylodendron Cowardii.

A —External view slightly reduced In the upper half some of the lozenge-shaped areas (foliar gaps) show the leaf-trace running up from the lower angle of the gap.

B.—External view from the other side. The irregular markings in the thickest portion of the fossil may represent the position of branch scars. Both views show a transverse mark where the fossil had been cut in two.

EXPLANATION OF PLATE II.

Fig. 1.—Secretory passage (mucilage? canal) from pith of *Tylodendron Cowardii* in transverse section. Several of the surrounding cells have undergone division. × 65.

Fig. 2.—Two adjacent secretory canals of the pith. × 65.

Fig. 3.—Apparent fusion of two adjacent canals. × 65.

Fig. 4.—Secretory canal showing repeated division of the surrounding cells of pith. The dotted lines indicate the walls which could not be clearly distinguished in the section. × 65.

Fig. 5.—Longitudinal view of secretory canal. The horizontal elongation of the parenchyma cells of the pith can be clearly seen. × 65.

Fig. 6.—Portion of transverse section across the inner margin of the vascular tissues showing a group of tracheids isolated in the parenchyma of the pith. × 150.

Fig. 7.—Radial longitudinal section through portion of vascular tissue showing two isolated scalariform tracheids to the inside (left) of the main mass of tracheids. × 150.

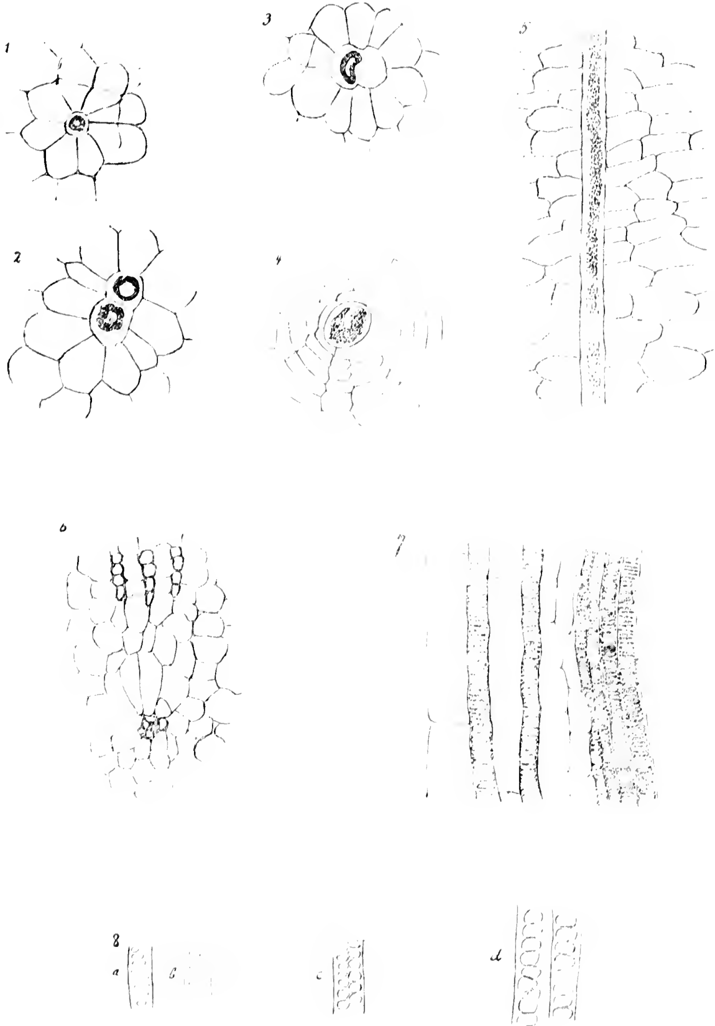
Fig. 8.—Portions of tracheids from radial longitudinal section showing various forms of pitting.

a. Horizontal pits.

b. Two rows of bordered pits and one "fused" pit at base. The latter closely resembles the pits seen in *a*.

c. Two alternate rows of pits.

d. Single row of pits. All × 230.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Manchester Memoirs, Vol. lvii. (1913), *No.* **19**.

XIX. Contributions to the History of Science (Period of Priestley—Lavoisier—Dalton), based on autograph documents.

By Kurt Loewenfeld, Ph.D.

(Read October 29th, 1912, and November 26th, 1912. Received for publication December 10th, 1912.)

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I. Dalton and his Contemporaries.

II. Priestley and Lavoisier.

I wish here to express my sincere thanks to Mr. Francis Jones, M.Sc., F.R.S.E., F.C.S., who kindly lent me the originals of *Pls. III.A* and *III.B* for reproduction, and to Mr. A. Sutton, who provided me with the rare woodcuts, *Text-figs.* 1 and 2, referring to Dalton. The other documents are out of my own collection.

Manchester Memoirs, Vol. lvii. (1913), No. 19.

XIX. Contributions to the History of Science (Period of Priestley—Lavoisier—Dalton), based on autograph documents.

By Kurt Loewenfeld, Ph.D.

(*Read October 29th, 1912, and November 26th, 1912. Received for publication December 10th, 1912.*)

I. Dalton and his Contemporaries.

It is an obvious fact that the history of science is a subject to which at present comparatively few turn their attention. Considering the many intellects which we find devoted to the advancement of science in every department, it is certain that lack of workmen cannot be the reason.

It might be suggested that in a time like ours, when new inventions and discoveries are made daily, it would be much more tempting to direct the searching eyes only into the future and to leave the past, which appears to the casual inquirer merely a land of obsolete facts and exploded theories, to its dusty existence in rarely opened folios.

When we hear how a great invention or an important discovery was made, our passing interest is aroused, but that good men should tarry and dig around the foundations of that most marvellous building of human science, instead of helping to raise its lofty towers, remains in the eyes of the majority waste of otherwise more useful energy. Nevertheless, is it really a waste? Is the history of science not a most important factor?

Every age has presented its own problems and has formed its own more or less true solutions. Methods have

November 29th, 1913.

to some extent changed but not so much that the methods of by-gone times, either theoretical or practical, could not be studied and used again with some advantage.

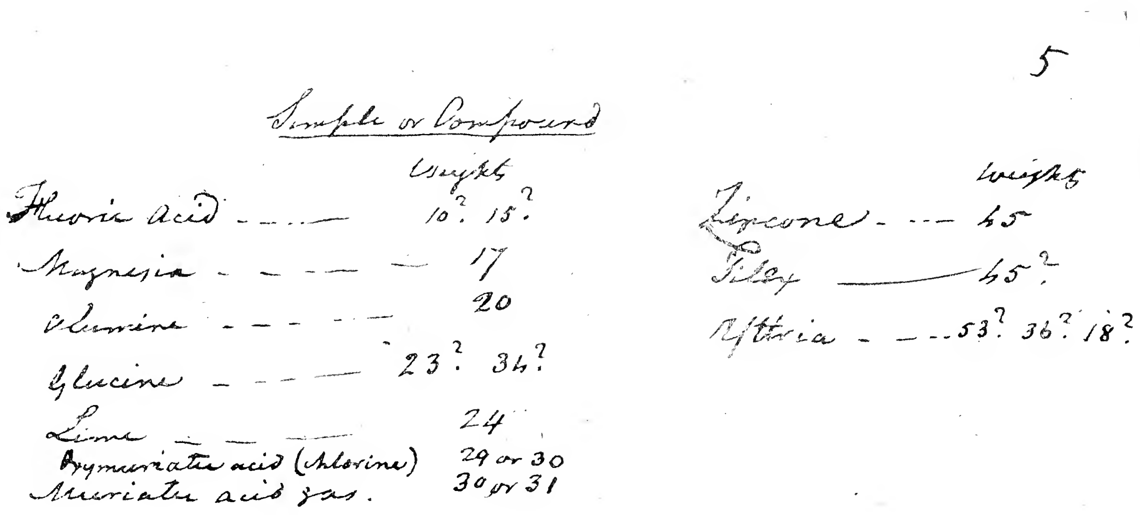
The value of the study of political history in mental culture is admitted. But how often is history well taught, or rather, how rarely?

It is impossible to understand modern times without a knowledge of the development brought about by the evolution of science, caused, accompanied and followed by the great technical inventions like cheap printing, and the triumph over the powers of nature, especially through the applications of steam and electricity and through the use of chemistry. The history of these developments, which at present is only casually taught, should form the basis from which modern universal history ought to be considered, and should be treated at least as extensively as all the Greek and Roman Wars. If we look around and see which methods are everywhere employed to foster historical studies, we find that in literature and politics the study of documents has proved itself invaluable. Every scrap of paper on which a great king, a great musician or writer, has set his pen is nowadays eagerly collected and forms—at least in many cases—the material not only for biographies but for history itself. It is to be regretted that similar methods are so seldom employed as far as the history of natural science is concerned.

I myself have tried for some years to form a collection of documents relating to the development of science, a few of which I shall refer to in detail.

I think I could not commence with a name more interesting to this Society than that of John Dalton.

The most important of my Dalton manuscripts is the table of atomic weights in Dalton's own handwriting. (See *Pl. I.*) The paper is not dated but it bears the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Simple or Compound

	Weight
Fluoric acid	10? 15?
Magnesia	17
Alumine	20
Glucine	23? 34?
Lime	24
Oxymuriatic acid (chlorine)	29 or 30
Muriatic acid gas.	30 or 31

	Weight
Zircone	45
Silex	45?
Yttria	53? 36? 18?

Specimen of John Dalton's handwriting.

(Draft of Atomic Table.)

watermark 1815. The fact that mention is made of Deutoxide of Hydrogen (Hydrogen peroxide H_2O_2), which was only discovered by Thenard in 1818, will compel us to date the handwriting not earlier than that year. On the other hand, it can be shown by comparison that this table was a draft for the last table published by Dalton in 1827. It is therefore evident that it was written between 1818 and 1827.

As Sir Henry Roscoe has reproduced in his 'New view of the Origin of Dalton's atomic theory' the first draft of the atomic figures, the original of which belongs to this Society, I thought it would be interesting to see the table in its final form.

With the name of Dalton is closely connected that of William Henry. William Henry, the son of Thomas Henry, who was the Secretary of the Literary and Philosophical Society, and its President from 1807 to 1816, studied medicine and became Physician to the Manchester Infirmary. Like his father, he was deeply interested in Chemistry. In this field, the law[1] named after him, communicated by him to this Society in 1803, has certainly to be accounted one of the impulses actuating Dalton in his atomic investigations. Henry also resembled his father in that he was a successful chemical manufacturer. The following letter shows him vacillating between scientific pursuits and practical progress :—

MANCHESTER,

18th August, 1817.

MY DEAR SIR,

It is impossible that I can feel otherwise than highly gratified by your friendly letter which has this morning reached me, and by the flattering suggestion which it contains. To be thought worthy,

[1] Henry's law states that the volume of a gas dissolved by a definite volume of a liquid is independent of the pressure.

by yourself and by my other friends at Glasgow, to fulfil the duties of a station of such importance and usefulness as the one which has lately become vacant in your university, is of itself a distinction which I little expected and which (whatever might have been the result of my becoming a candidate) impresses me with the most lively sense of obligation. As a source of emolument, the office (could I have succeeded to the extent of my own wishes in performing its duties) would have been an object of great consequence to me. But though I estimate most highly its value in this way, as well as in conferring scientific rank, yet I am compelled, by a variety of circumstances which for some years to come must bind me to this spot, to forego all intentions of proposing myself for a situation which, in almost every respect, would have been more agreeable to my taste and habits than the sphere in which I am now moving. For some time past, I have been induced, by habitual delicacy of health, to decline rather than to seek an extension of medical practice, and as my family is a pretty large and increasing one, I have made, whenever I have been tolerably well, great exertions to extend my chemical manufacture. These exertions have been attended with all the success I could have expected, and from time to time I have been induced to lay out sums of money in buildings and utensils, the aggregate of which is to me of serious moment. I am of opinion, therefore, that it will be more consistent with my interest and happiness to persevere steadily in the course I am now pursuing than to yield even to so strong a temptation as that which you hold out. Added to this, the very disqualification which unfits me for medical practice, would be a serious bar to my performing the duties of the proposed situation with that regularity which would be absolutely necessary.

Twice within the four or five last years I have had attacks of hæmoptysis, slight it is true, but still sufficient to render any situation ineligible in which public speaking would be a necessary duty. That I may not, therefore, be the means of interfering with the views of any other candidate, I lose no time in declining your very kind and flattering suggestion and in assuring you and my other friends who have concurred in it that I am deeply sensible of the esteem by which it has been prompted.

Now that the office is vacant, would it not be advisable to require that the future holder of it should not practise medicine? All the sound and formidable reasoning of Professor Playfair[2] in the controversy respecting the mathematical chair of Edinburgh seems to me to apply with equal weight to a chemical lectureship. In the courses of Lectures which I formerly gave, I found employment for my whole time as long as they lasted, and though much may no doubt be done to abridge the labour by arrangements similar to those so successfully practised by Dr. Hope,[3] yet I cannot conceive even moderately extensive medical practice to be otherwise than incompatible with daily lectures on any experimental science, and with that devotion of mind and ardent enthusiasm which a public teacher ought to feel and to kindle to a considerable degree in his hearers.

Believe, etc., etc.,

W. HENRY.

To Dr. Bown,
Miller Street,
Glasgow.

[2] John Playfair (1748-1819), Mathematician. Joint Professor of Mathematics at Edinburgh 1785-1805; became in 1805 Professor of Natural Philosophy. Grandfather of the famous chemist, Sir Lyon Playfair.

[3] Thomas Charles Hope (1766-1844), Professor of Chemistry at Glasgow and Edinburgh.

The end of the letter is most interesting as it throws a sidelight on University teaching in 1817, when the Professor of Chemistry at a University such as Glasgow had also to fulfil the duties of a medical practitioner.

The following letter is remarkable as it shows the close intimacy which existed between the writer and Michael Faraday. Henry's admonitions were not thrown away, as Faraday at this time was busy with his immortal discoveries concerning the electrical current. As the letter treats to a great extent of personal matters, an extract may be sufficient :—

MANCHESTER, 8/2/1831.

. . . . Having got your second edition[4] off your hands, you must permit me to say that I hope you will direct your next exertions to some of those elevated topics of chemical philosophy to which you have established your title to aspire. In the exercise of your public duties in the Royal Institution,[5] you must necessarily, while treading the ground which has been already cleared, sometimes cast your eye beyond its boundaries, and catch glimpses of extensive tracts on which nothing more than a dawning light is yet shed. It is impossible for anyone (even for a person like myself, whose energies of thought and purpose are on the wane) not to be warmed into something like enthusiasm when fancy pictures the glories that are yet to be won in the fields of chemical science. These bright though distant prospects will, I trust, tempt you to open and pursue paths that may lead you to great discoveries, to the benefit of science, and to the increase of your own honourable fame . . . .

WILLIAM HENRY.

To Michael Faraday.

[4] Viz., of "Chemical Manipulation." First edition in 1827; second ed., 1830.

[5] Faraday had become, in 1813, Davy's assistant; in 1825, Director of the Laboratory of the Royal Institution, and had given his first course of lectures in 1826 at the Institution, followed by innumerable other lectures.

William Henry had a great inclination towards historical studies, and, besides publishing some interesting papers on Priestley, etc., collected material for a history of chemical discovery. Unfortunately, this material never appeared in print. It was, however, the lot of his son, William Charles Henry, to enter the field of historical authorship by the 'Memoirs and the Life and Scientific Researches of John Dalton,' which was published ten years after Dalton's death by the Cavendish Society. The author thought it necessary (see Preface) to apologise for the long delay. He states, as a reason, that Dalton bequeathed to him and his three co-executors 'all his philosophical, scientific and literary manuscripts and correspondence, to be disposed of as they may judge most fit.' Henry states further that this clause of the will was not communicated until 1844. He goes on to say 'Regarding them (he refers to the clause of the will) as significant of my venerable friend's intention that I should act as his literary executor, and should write some account of his life and discoveries, I commenced shortly after my return (namely, from a journey to Italy in 1844-5) to prepare for the task.'

This printed statement is flatly contradicted by a letter, in Henry's own hand, in which he says :—

HAFFIELD, NEAR LEDBURY,
HEREFORDSHIRE,
January 22nd, 1838.

MY DEAR SIR,

Your obliging letter proposing to me to undertake a Report for the British Association was placed in my hands only a few days ago, on the occasion of my visiting Manchester, the person who superintends my affairs there having unaccountably neglected to forward it to me here. I do not feel myself sufficiently conversant with the more recent accessions to that branch of science to venture for the present to accept the proposal you have done me the honour to make. I have also another object in view to which I am desirous of devoting myself uninterruptedly. My excellent friend Dr. Dalton, after the severe seizure which threatened his life early last year, bequeathed his scientific papers and unpublished manuscripts to my care, and I should therefore feel it my duty in the work which at his advanced age and impaired health cannot be very remote to attempt some biographical notice of him and some analysis of his scientific discoveries. When I last saw him his physical health was materially improved, but his memory and articulation have been much weakened.

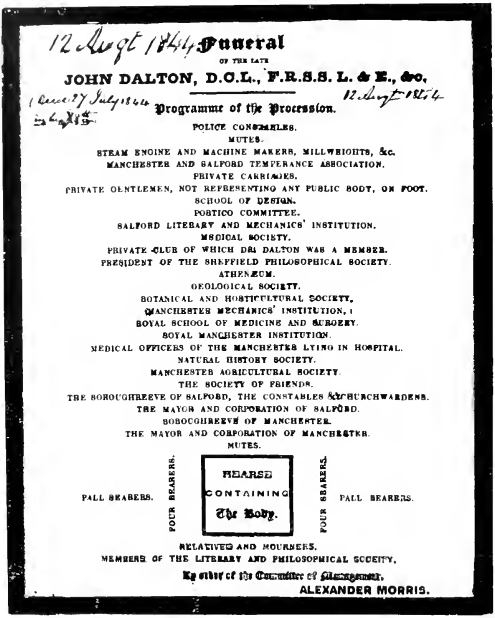
It is now some months since I ceased to reside in Manchester. I find the leisure and retirement of a country life infinitely more favourable to intellectual pursuits than the excitement and turmoil of a great commercial community.

Believe me, my dear Sir,
With sincere respects,
Yours most faithfully,
WM. CHARLES HENRY.

The Revd. James Yates,[6] M.A., etc.,
49, Upper Bedford Place, London.

One may consider this little incident hardly important. But still I thought it worth mentioning, as it gives us an idea as to the general trustworthiness of Henry as a historian. It will be remembered that the still unsettled question regarding the origin of the atomic theory depends, to a great extent, on statements made in this biographical book, as William Charles Henry was a pupil of Dalton, one of those fortunate men whom Dalton, for a fee of 1s. 6d. per hour, introduced into the intricacies of chemistry.

[6] James Yates (1789–1871), Unitarian and antiquary, secretary to council of British Association in 1831.



12 Augt 1844 **Funeral**

OF THE LATE

JOHN DALTON, D.C.L., F.R.S.S. L. & E., &c.

(Died 27 July 1844) 12 Augt 1844

Programme of the Procession.

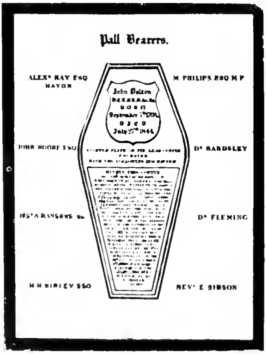
POLICE CONSTABLES.
MUTES.
STEAM ENGINE AND MACHINE MAKERS, MILLWRIGHTS, &c.
MANCHESTER AND SALFORD TEMPERANCE ASSOCIATION.
PRIVATE CARRIAGES.
PRIVATE GENTLEMEN, NOT REPRESENTING ANY PUBLIC BODY, ON FOOT.
SCHOOL OF DESIGN.
PORTICO COMMITTEE.
SALFORD LITERARY AND MECHANICS' INSTITUTION.
MEDICAL SOCIETY.
PRIVATE CLUB OF WHICH DR. DALTON WAS A MEMBER.
PRESIDENT OF THE SHEFFIELD PHILOSOPHICAL SOCIETY.
ATHENÆUM.
GEOLOGICAL SOCIETY.
BOTANICAL AND HORTICULTURAL SOCIETY.
MANCHESTER MECHANICS' INSTITUTION.
ROYAL SCHOOL OF MEDICINE AND SURGERY.
ROYAL MANCHESTER INSTITUTION.
MEDICAL OFFICERS OF THE MANCHESTER LYING IN HOSPITAL.
NATURAL HISTORY SOCIETY.
MANCHESTER AGRICULTURAL SOCIETY.
THE SOCIETY OF FRIENDS.
THE BOROUGHREEVE OF SALFORD, THE CONSTABLES & CHURCHWARDENS.
THE MAYOR AND CORPORATION OF SALFORD.
BOROUGHREEVE OF MANCHESTER.
THE MAYOR AND CORPORATION OF MANCHESTER.
MUTES.

PALL BEARERS. FOUR BEARERS. | HEARSE CONTAINING The Body. | FOUR BEARERS. PALL BEARERS.

RELATIVES AND MOURNERS.
MEMBERS OF THE LITERARY AND PHILOSOPHICAL SOCIETY.

By order of the Committee of Management,
ALEXANDER MORRIS.

A.



B

Official invitation, and arrangement of pall-bearers, for Dalton's Funeral

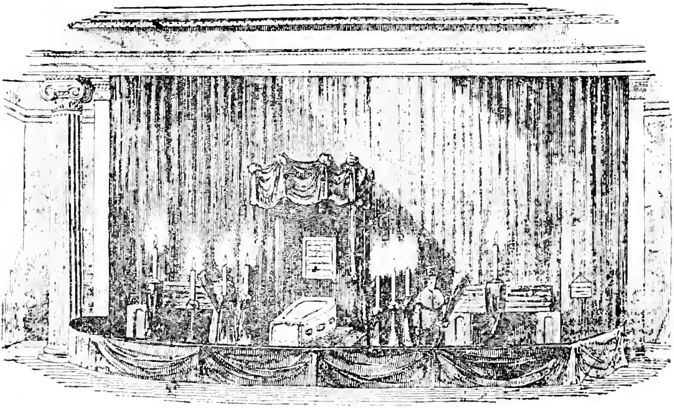
Henry mentions further that Dalton's friend and his co-trustee, Peter Clare, was unwilling to part with the documents and kept them until his death. Supposing Henry is right here, it is certain that we cannot blame the faithful Peter Clare, especially as it appears from Dalton's will that Henry had to co-operate with the other trustees, insomuch as concerned publication. Henry nevertheless insisted on doing the work by himself, and I cannot say that he has succeeded admirably, considering the invaluable material he had in Dalton's notebooks, etc. It is only recently that Sir Henry Roscoe and Dr. Harden showed what a wealth of historical information was slumbering in those large files and used it to a great extent in the book already mentioned.

Dalton never recovered from the illness referred to in the previous letter, and eventually passed away on the 27th July, 1844. A public funeral was decided upon, and the then Mayor of Manchester, Mr. Alexander Kay, took the matter especially in hand. The Mayor of Manchester had issued a note which intimated to the citizens "that by closing their warehouses and shops from eleven until one o'clock . . . . they will best show their respect for the late Dr. Dalton." The report in the "Manchester Guardian" states that "the recommendation of the Mayor was very generally complied with," and that ladies and gentlemen in mourning filled every window where the procession passed.

I reproduce the official invitation (See *Pl. II. A*) as well as the arrangement of the pallbearers. (See *Pl. II. B*). You will notice that the Mayor of Manchester was one of them; others were relations of Dalton, members of this society, members of the medical society, and of many other societies, as will be seen on the illustration facing this page.

The lying-in-state took place in the Town Hall. A contemporary newspaper gave a rough woodcut of this memorable event, and this cut is reproduced below.

The room was artificially darkened, the whole matter being done with great pomp, and it is recorded that more than 40,000 people visited the room.



Text-fig. 1.

I have not been able to find the name of the paper in which this and the following cut appeared, the two pictures being only preserved in a scrap book.

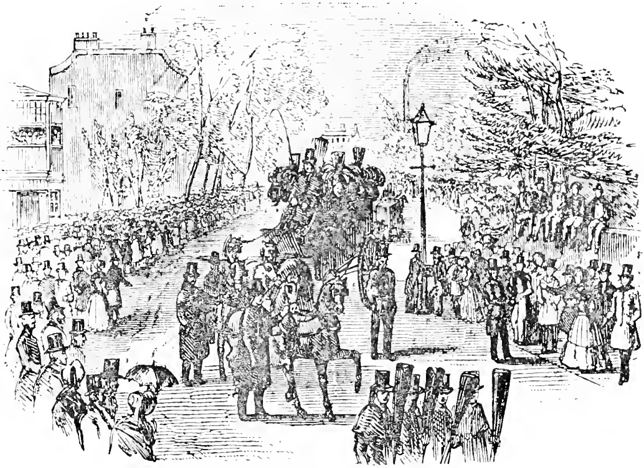
The funeral itself took place on Monday, the 12th of August. The second of these woodcuts gives an illustration of the funeral passing through the streets of Manchester.

The following account of this funeral appeared in *The Friend*, a Quaker paper. It is most remarkable—

firstly, because it gives a good description; secondly, because of the spirit in which it was written :—

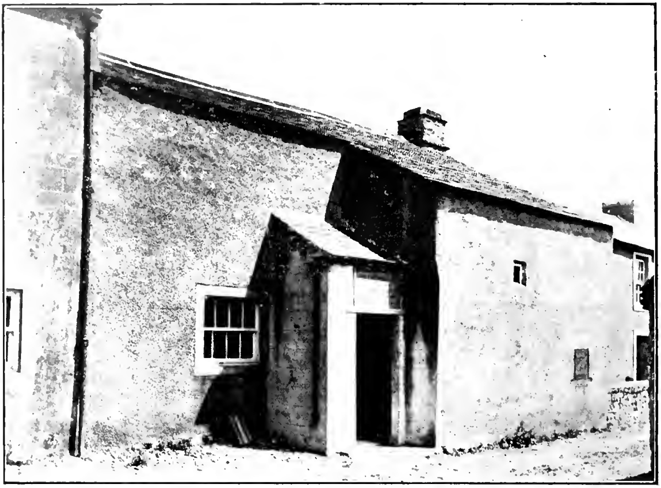
Extracted from *The Friend*, published in London, 9th month, 1844.

"The circumstances attending the funeral appear to us of so painful and objectionable a character, that

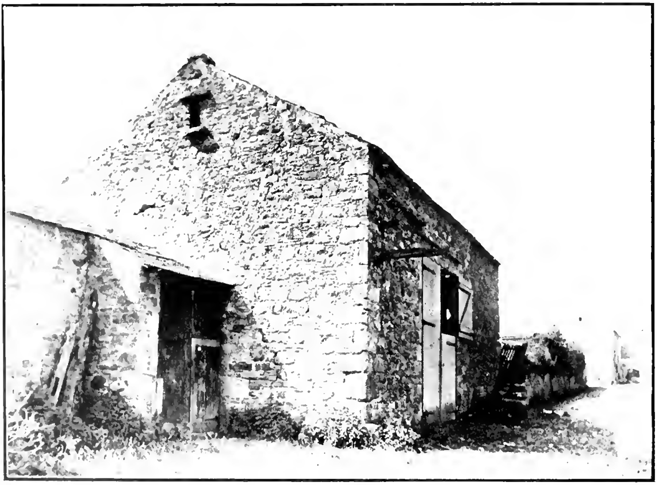


Text-fig. 2.

we can scarcely forbear offering a few remarks upon them. John Dalton had appointed three executors two of whom, at the time of their appointment, were members of the Society of Friends, and one remains so still. We premise this much, to show that the circumstances we are about to allude to could not have arisen from the ignorance of the parties concerned of the principles and practices of our religious body, or of the deceased as a member of it. We learn from the public papers that so far from the last solemn rites to the departed being conducted in that quiet and unostentatious manner which our Society has ever held to be in accordance with the simplicity of the Gospel, and which we believe would have suited best with the feelings of the deceased philosopher, pomp and pageantry, and the outward semblances of sorrow, were made to follow the remains of the highly-gifted, yet simple-minded man. 'The body,' says one of the papers, 'lay in state in the Town Hall, which was hung with black cloth, and numbers of persons came to view the coffin.' Then follows a detailed account of the long train of carriages, mourning coaches and mutes, and the array of plumed horses and craped mourners and supporters of the pall that attended an unassuming *Quaker* to his last resting place. Would the proofs of grief have been less sincere, the respect intended to be shown less real, had those who took upon them the conduct of this funeral arranged it in some degree in accordance with the views of the deceased? It is to us a serious thing, and an instance of which we do not recollect a precedent, that the ashes of a man, who, through a long life, supported certain important principles, should be made to participate in their violation. We hear that the nearest relatives of the deceased, who were members of our Society, objected strongly against the pompous ceremonial; and we are glad to find that some of the Friends of Manchester Meeting signed and forwarded a strong protest to the executors on the subject, and that they have likewise made a public avowal of their objections in one of the Manchester papers. We understand that the so-called Church of England burial service was read over the remains, and whilst we are very far from desiring in any way to judge our brethren, and can readily sympathise with their desire to show respect to one so honoured and



A. Dalton's birthplace.



B. Barn used by Dalton as a school.

esteemed, we were a little surprised to find that so many of our members stood by whilst the ceremony was performed."

I think this article expresses nothing better than the unfriendly spirit of its author, and one might be easily induced to believe that the Quakers, as a body, were opposed to the public honours which were given to Dalton. It must be remembered that Peter Clare, who was himself a member of the Society of Friends, not only identified himself with the funeral, but took part in all the meetings and thus sanctioned the proceedings. As a matter of fact, it is most likely that only a very small proportion of the Quakers took offence at the public honours paid to Manchester's great citizen.

A remarkable contrast to the pomp which characterised Dalton's exit out of the world is shown in the accompanying illustration of his humble birthplace, Eaglesfield. (See *Pl. III.A*).

Dalton was all his life a schoolmaster. The story is well known that once a great French chemist called on Dalton, who was at the time giving a lesson to a boy. Dalton was not in the least disturbed, but said quietly, "Please take a seat until I have put this lad right about his arithmetic." But even this episode shows Dalton already well advanced in his career as a teacher. Along with his brother he started a school in a barn (See *Pl. III.B*) on the 28th of March, 1785, at Kendal. He was a teacher up to the last, teaching not only little boys, but the chemists of all countries the foundations of their science.

Later on, in about 1853, a still more substantial tribute was paid to the memory of John Dalton. A public fund, amounting to no less than £5,312, was raised by a committee under the chairmanship of William Neild. Underneath is a reproduction of the invitation for the subscription.

SUBSCRIPTION

IN AID OF THE FUND TO DEFRAY THE EXPENSES OF

THE FUNERAL

OF

THE LATE DR. DALTON;

TO ERECT A

SUITABLE MONUMENT

OVER THE MORTAL REMAINS OF THIS ILLUSTRIOUS PHILOSOPHER AND EXEMPLARY CHRISTIAN,

IN THE CEMETERY AT ARDWICK;

AND ALSO TO FOUND A

PROFESSORSHIP OF CHEMISTRY,

IN SOME PUBLIC INSTITUTION IN MANCHESTER, TO BE NAMED THE "DALTONIAN PROFESSORSHIP,"

One object of which shall be to illustrate the ATOMIC THEORY, and the Discoveries of DALTON in connection with other branches of Physical Science.

SUBSCRIPTIONS.

Alderman Neild	£52	10	0
Alderman Kershaw	52	10	0
Alfred Binyon	52	10	0
Dr. Bardsley	21	0	0
John Moore	21	0	0
Dr. Fleming	21	0	0
Peter Clare	26	5	0
James Heywood, F.R.S.	52	10	0
The Mayor	21	0	0
Wm. R. Callender	21	0	0
Laurence Buchan	21	0	0
Joseph Compton	52	10	0
Richard Lane	10	10	0
Benjamin Joule	21	0	0
Edmund Peel Thomson	52	10	0
James Thomson, F.R.S.	52	10	0
Alexander Bannerman	21	0	0
John Bannerman	21	0	0
Henry Bannerman	21	0	0
George Murray	52	10	0
Alderman Murray	21	0	0
Alderman Burd	21	0	0
Alderman Brooks	52	10	0

Dalton and Charles Henry carried on simultaneously with Davy and others experiments on ammonia. At that time this compound excited the greatest interest. It had been discovered in 1774 by Priestley and was as far back as 1785 correctly analysed by Claude Louis Berthollet, but nevertheless many chemists, including Davy and Henry, were of opinion that ammonia contained oxygen. About 1808 Davy was very busy in applying the newly discovered Voltaic Pile to all sorts of compounds, with a view to ascertaining their composition. Ammonia had proved especially difficult to decompose. At this time the discovery of ammonium amalgam threw new light on the matter. The experiments concerning it are described in the following letter by Charles Blagden [7] to Thomas Thomson.[8]

> ... The chemists here have not succeeded in the French experiment of obtaining the basis of potash pure in the dry way; they get only an alloy of iron and the basis. But two Swedish chemists, Berzelius[9] and Pontin,[10] have applied the affinity of the bases for mercury very advantageously, and by means of it decomposed ammonia calx, barytes, etc. The substances to be decomposed must be put into contact with the mercury and the galvanic current forced through. Carbonate of ammonia being laid upon the quicksilver, the latter is soon made by the galvanism to swell up to several times its original bulk, and to assume the appearance of an amalgam, which being thrown into water gradually shrinks back to its original size, and becomes running quicksilver as before; it is a beautiful experiment.
>
> The Swedes suppose that the hydrogene and azote of the ammonia combine to form a metal, which thus unites to the mercury. Mr. Davy thinks rather that hydrogene and azote are both metals in a state of vapour, which both unite to the mercury and form a triple compound with it. Mr. Davy has successfully repeated all the experiments, and has decomposed in that manner 6 earth-baryte, lime, magnesia, silex, and alumine. . . . .
>
> London, 7/7/1808. CHARLES BLAGDEN.

[7] Sir Charles Blagden (1748-1820), physician, Secretary of the Royal Society.

[8] Thomas Thomson (1773-1852), chemist. Supporter of Dalton's theory and author of a 'History of Chemistry.'

[9] Jöns Jacob Berzelius. The great chemist. (1779-1848.)

[10] Magnus Pontin of Pontin (1781-1858). Swedish physician and author. Collaborated occasionally with his friend Berzelius, whose biography he wrote.

Davy's bold idea to group hydrogen among the metals appears here for the first time. I believe I am right in describing this theory as now generally accepted. The above letter shows that these documents are to be handled with a certain caution. It would be erroneous to say that Berzelius and Pontin were the first discoverers of the amalgam of ammonia. It was discovered in the first instance by Johann Thomas Seebeck. The small letter reproduced in *Pl. IV.* is by Davy, and is addressed to Coleridge, who, like Southey, was an intimate friend of the great philosopher, and held the opinion that "if Davy had not been the first chemist he would have been the first poet of his age."

A rather unfriendly criticism of Davy is contained in the following letter by Berzelius.[11]

STOCKHOLM, le 3 Sept., 1847.

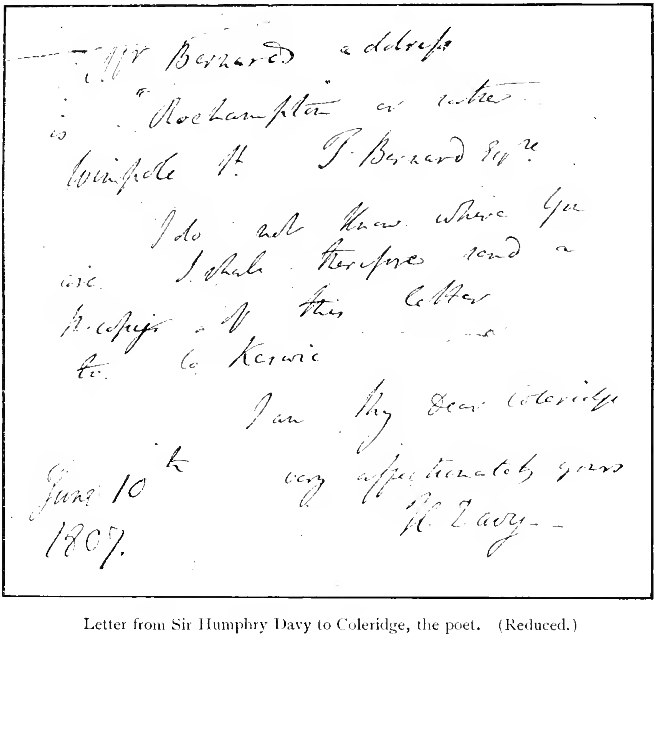
MON TRÈS CHER AMI!

J'ai à vous presenter des remercimens de l'Académie des Sciences d'abord pour votre brochure: Cause du deraillement des Waggons, etc., et ensuite pour l'ouvrage intitulé: Application de la géométrie descriptive aux ombres à la perespective, etc. Si je n'ai pas plutot repondu à votre aimable lettre du 21 Fevrier, c'est que je voulais vous annoncer l'arrivé de ce dernier, qui n'arriva à Stockholm, que pendant mon absence por assister à la reunion des Naturalistes Scandinaves pendant l'été passé.

Ce que vous me dites dans votre lettre pour rapport à Mr. Dumas,[12] tout le monde qui vient de

[11] About the relations between Berzelius and Davy, *vide* "Jacob Berzelius, Selbstbiographishe Aufzeichnungen." Edited by H. G. Söderbaum, Leipzig, 1903, page 54 and passim.

[12] Jean Baptiste Dumas (1800-1884), eminent French chemist.


Mr Bernard's address
is Roehampton or rather
Wimpole St. T. Bernard Esqre.
I do not know where you
are. I shall therefore send a
copy of this letter
to Keswick
I am my dear Coleridge
June 10th very affectionately yours
1807. H. Davy

Letter from Sir Humphry Davy to Coleridge, the poet. (Reduced.)

Paris me le repete. J'ai fait l'observation que les chimistes qui n'ont pas eu l'occasion de faire leurs études dans des ecoles savantes, et qui par des circonstances, dont ils n'ont pue être les maitres, ont fait leurs études dans des pharmacies, où ils ont été installés dans leur premiere jeunesse, ne peuvent ensuite jamais atteindre à cette culture scientifique générale, qui est le resultat des études dans une ecole savante. Ce n'est qu'à un age plus avancé, qu'ils s'appercoivent de ce qui leur manque, mais alors ils sont tellement accoutumé à ne s'occuper que de ce dont l'application et l'utulité saute aux yeux, il leur est rebutant de commencer par le commencement, ils se procurent dans d'autres sciences quelques notions superficielles, dont ils ignorent les bases, et s'ils viennent à jouer un rôle dans la chimie, ils se servent à tort et à travers de ces notions superficielles, pour faire croire au monde qu'ils ont étudié au fond. Je puis vous citer, outre Dumas, encore Liebig et une capacité chimique encore plus respectable, l'illustre Humphry Davy, qui dans nos jours ont été des preuves de la vérité de cette observation. Comme ils n'ont aucune peur por s'en servir dans leurs travaux, ils brillent par là auprès des gens du monde, sans s'inquiéter de ce que le petit nombre de vrais erudits y reconnait de l'ignorance et de la charlatanerie.—C'est de cette manière, que se produisent des reputation populaires brillantes, mais que la personne elle même, survit, si toutefois l'impitoyable Atropos n'employe pas trop tot les ciseaux.

Notre ami Palmstedt[13] a été obligé de passer tout l'été à Stockholm, ayant été nommé par le Roi à diriger l'exposition industrielle, qui eut lieu au mois de Juillet à la Capitale. D'après le jugement de tous ceux qui ont eu occasion de voir tout cette exposi-

[13] Carl Palmstedt (1785-1870). Swedish chemist. Headmaster of School of Technology at Gothenburg. Helped Berzelius in research work in 1816 and travelled later on with him.

tion, en elle meme fort pauvre, que les précédens plus riches, rendent justices aux efforts de notre ami. Il est encore ici, attendant le retour prochain du Roi, pour rendre compte de sa commission. Il me prie de le rappeler à votre souvenir et ajoute que probablement il visitera Paris de nouveau l'été prochain, où il aura le plaisir de vous trouver ches vous.

Je vous prie de presenter mes respects à Madame Olivier.

Votre ami devoué

JAC. BERZELIUS.

Monsieur le Professeur Th. Olivier,[14]
à Paris.

Reference is also made to Davy in the following letter by Christian Friedrich Schönbein. The famous discoverer of ozone was born in 1799, and thus the letter, which is dated 1826, was written when he, as a young man, spent a short time in England. The remarks on scientific and other institutions in England and abroad are, though perhaps not quite just, certainly interesting :—

EPSOM BEI LONDON,
d. 20 Juny, 1826.

VEREHRTESTER HERR HOFRATH!

Ich nehme mir die Freiheit Herrn Berdonnet aus Paris hiermit bei Ihnen einzuführen u. denselben zu geneigter Aufnahme zu empfehlen. Er studierte in Paris u. hat sich namentlich als Physiker Chemiker u. Mineralog ausgezeichnet; und jetzt ist er auf einer Reise begriffen, deren Zweck dahin geht, Deutschland und einige andere Länder des Kontinents in mineralogischer u. bergmännischer Hinsicht kennen zu lernen. Ich kenne Herrn Berdonnet nicht persönlich, nur mittelbar durch einen seiner Freunde, der zugleich

[14] Théodore Olivier. French Mathematician; died 1853. Professor at Ecole Polytechnique.

auch mein Freund, u. welcher mich um Empfehlungen für ihn nach Deutschland bat.

Sie wissen ohne Zweifel dass ich seit einig. Zeit Keilhau verlassen habe u. in England bin. Ich lebe seit meiner Ankunft in Epsom einem kleinen Landstädtchen 15 engl. Meilen von London, auf dem Kontinent glaube ich nur den Apothekern dem Namen nach bekannt. Im nächsten halben Jahre werde ich in London meinen Aufenthalt nehmen, um daselbst die naturwissenschaftl. Anstalten zu benüzen, woran übrigens nicht viel ist, namentlich wenn mit den Parisern verglichen. Alles hängt in England in dieser Beziehung von Privaten ab, u. ausser dem brittischen Museum, was sich im Gebiet der Naturwissenschaften nur auf Naturgeschichte beschränkt ist keine vom Staat unterstützte naturwissenschaftliche Anstalt in London. Die royal and mechanical Institution z. B. in denen Vorlesungen über verschiedene Zweige der Naturwissenschaften gehalten werden, sind privat u. die meisten Naturforscher Privatleute, die von ihren Renten leben, u. gewöhnlich sehr reiche Gentlemen sind ; wie Davy, Wollaston, etc.

Auf dem Kontinente ist England hinsichtlich seines wissenschaftlichen Standpunktes weit überschätzt, u. so weit ich es kenne kan es in dieser Beziehung in keine Parallele mit Frankreich oder Deutschland gestellt werden. Nichtsdestoweniger aber ist England ein höchst interessantes und originelles Land, reich an den grossartigsten Erscheinungen, so dass kaum ein Land auf dem Kontinent damit glichen werden konnte. Wenn man zu ersten male den englischen Boden betritt, man glaubt sich in eine andere Welt versetzt ; in nicht wenigen Fällen stösst man auf die angenehmsten Kontraste (vorzüglich mit Deutschland) u. namentlich vermisst man gleich beim Eintritt mit gröstem Vergnügen das noble Institut, auf dem Kontinent Polizei genannt, u. von der

Weisheit oder vielmehr Klugheit der dasigen Staatsmänner als eine Anstalt betrachtet, ohne welche die Europäische Welt nicht existieren könnte. Sie mögen Recht haben.

Im nächsten halben Jahre wird einer meiner Freunde in England lebend u. ich uns die Freiheit Abhandlungen an die Facultät in Jena einzusenden, um uns zu Doctoren creieren zu lassen, wenn wir anders nicht auf der philosophischen Wage zu leicht befunden werden. Da nichts in der Welt umsonst geschieht, nicht einmal das Doctormachen, was doch ein so rein geistiger Aktus ist, so würden Sie mich sehr verbinden wenn Sie die Güte hätten mir zu schreiben oder auf irgend eine andere Weise mich wissen zu lassen, wie viele Pfund Sterlinge wir unseren Geisteskindern mitzugeben haben, um im Falle sie als philosophische Kinder erkannt und aufgenommen werden, sie zu befähigen, das Zeugniss für unsere Vaterschaft zu erhalten, was doch wohl allein bei der ganzen Sache als zahlungsfähig betrachtet werden kann.

Hochachtungsvoll empfiehlt sich Ihnen,

Ihr ergebenster,

C. F. SCHÖNBEIN,

at Dr. Mayo's, Epsom, Surrey, England.

Herrn Hofrath Doebereiner,[15]

Wohlgebohren, in Jena,

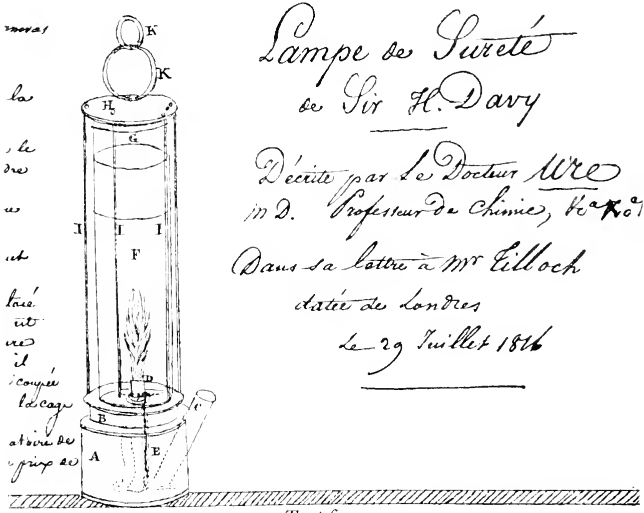
Sachsenweimar.

Sir Humphry Davy appeared, then, a leisurely wealthy gentleman to this little pharmacist, who ultimately became himself one of the most original thinkers in chemistry. Schönbein probably did not know the

[15] Johann Wolfgang Doebereiner (1780–1849), German chemist. Discovered the property of spongiform platinum to inflame hydrogen.

early history of Sir Humphry, who worked his way from a surgeon's assistant to the foremost place in English science—the presidency of the Royal Society.

An interesting drawing, which represents Davy's most important technical discovery, namely, his safety-lamp, is reproduced here. (*Text-fig.* 3.)

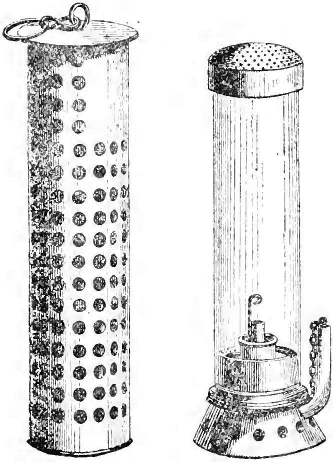


Text-fig. 3.

The drawing is taken from a contemporary letter of Andrew Ure,[16] in which he sent a description of the lamp to France. All the documents belonging to the history of this invention are noteworthy on account of the keen struggle which once existed between the followers of Davy and those of George Stephenson, who maintained that a

[16] Andrew Ure (1778-1857), chemist. Author of the well-known "Dictionary of Arts, Manufactures and Mines."

similar invention on the same principles had been made by the great railwayman This illustration (*Text-fig.* 4)

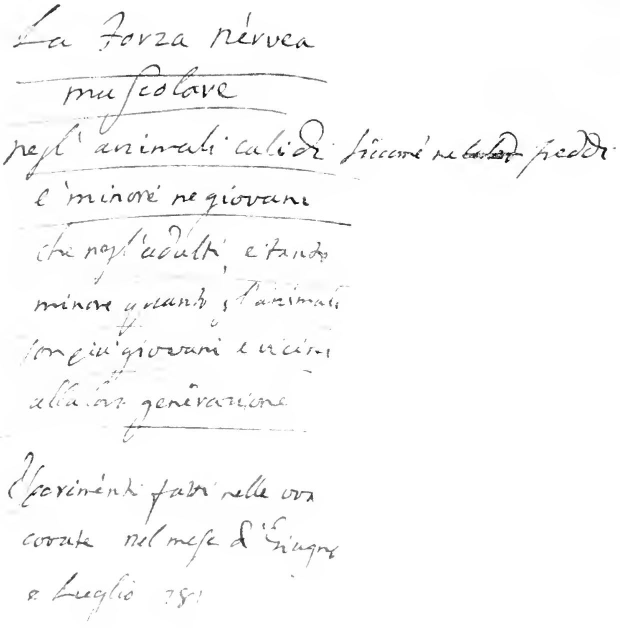


Text-fig. 4.

shows Stephenson's lamp which, though certainly giving evidence of the same principles and invented before Davy's lamp, is also certainly much less useful from a technical point of view.

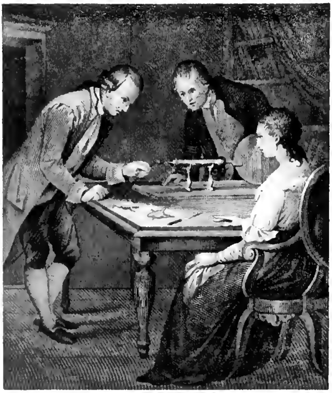
The great work of Davy in introducing the use of the electric current into chemistry has already been mentioned. Considering the enormous influence which the discovery of the galvanic current had on that period of chemistry to which I have confined myself thus far, namely, the first decades of the nineteenth century, a few documents relating to this discovery will be worthy of attention.

Galvani himself was utterly mistaken in the explanation he tried to give. He sought for the source of


La Forza nervea
muscolare
negl' animali calidi siccome ne ~~[illegible]~~ freddi
è minore ne giovani
che negl' adulti e tanto
minore quanto gl' animali
son più giovani e vicini
alla lor generazione

Esperimenti fatti nelle ova
covate nel mese di Giugno
e Luglio 781

A. Specimen of Galvani's handwriting. (Reduced)



B. Galvani in laboratory.

electricity in the muscles, and in the document reproduced, *Pl. V. A*, you see some lines written in Galvani's handwriting in which he says that the nerve-power is smaller in old animals than in young animals.

It was reserved for the great Alessandro Volta to find the right theory of electricity.

The discovery of galvanic electricity was made according to tradition as follows: Galvani's wife, an invalid, was advised to eat frogs' legs. These legs her husband prepared. He left the room after having skinned some. The legs on the table were touched by somebody with a knife at the same time as by chance Galvani's assistant turned the handle of an electrical machine. Thus a shock was communicated to the legs, the twitching of which was noted by Galvani's wife, who informed her husband. The scene of discovery is represented in an old engraving reproduced, *Pl. V. B.*

On *Plate VI.* there is a page from Volta's notebook, in which various bodies are arranged as to their electric qualities. This is one of the early drafts belonging to Volta's inquiries into the electric nature of various bodies which were crowned by the invention of the famous pile.

But how little Volta was recognised, at least in his own Fatherland, becomes evident in the following letter. This letter alters all the dates in biographies of Volta, which say that he retired in 1804, whereas this letter, wherein he clearly states that he had already retired, was written in 1803. He was, as a matter of fact, again given a professorship in Padua, in 1815, by the Emperor Franciscus of Austria. But how could a man be sufficiently honoured who could rightly say about himself 'Galvanism, the new branch of science, which I have almost created.'

The letter runs as follows :—

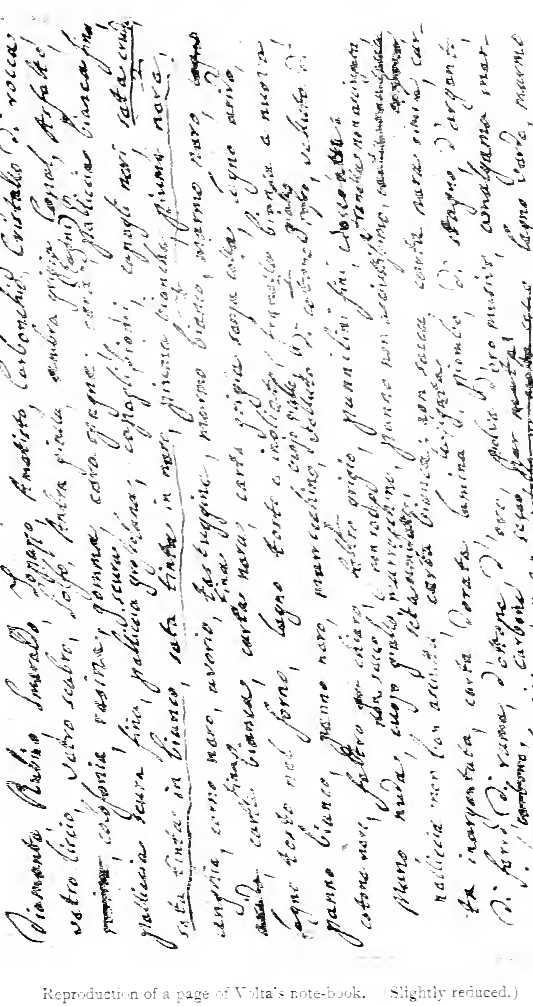
à Côme ce 25 Mars 1803.

MONSIEUR,

Je vous ai écrit il y a quelque mois une longue lettre, dans laquelle je vous communiquois quelques resultats de mes dernieres expériences électrométriques et concernant les charges portées aux batteries (composées de bouteilles de Oxyde) par mes appareils électromoteurs. Je me plaignois dans cette meme lettre de l'interruption de la correspondance avec Mr. le Prof. Pfaff,[17] que je ne savois à quoi attribuer, lui ayant écrit plus d'une fois depuis que j'avois reçu ses dernieres lettres il y a plus d'un an, et n'ayant pas eu de reponse : je vous priois de vouloir vous informer d'ou vient ce silence de sa part. ou si les lettres ne sont perdues : enfin de retablir par votre courtoisie cette correspondance précieuse. Je vous faisois des instances encor plus pressées pour une correspondance avec vous, sur-tout pour ce qui concerne le *Galvanisme*[18] ainsi dit (qui n'est qu'un nouveau moyen d'électrisation comme j'ai toujours soutenu, et tous les Physiciens ont dù enfin reconnoitre), et l'application de cette électricité à la medicine : attendu que vous recueillez tout le meilleur avec tant de diligence et de choix, et en faites part promptement aux lecteurs de vos excellent Annales de Physique. Pour cette correspondance, que je vous demande, il me suffiroit de recevoir sans rétard les numeros de ce Journal, et non pas de six en six mois, et même plus tard, comme il est arrivé jusqu'ici par les expéditions que m'en fait le libraire Barth de Leipzic, de sorte que je suis encore à

[17] Christoph Heinrich Pfaff (1772-1852). German physician and author of important books on electrical and chemical research.

[18] It is noteworthy that Volta himself coined the word "Galvanisme." At least, it appears in literature for the first time in one of Volta's papers. *Vide Gren. Neues Journal. III.*, 1796.



Reproduction of a page of Volta's note-book. (Slightly reduced.)

attendre tous les numeros depuis septembre. Je vous proposais donc dans la lettre susindiquée que je consignai à la poste (et que je crains que vous n'ayez pas reçue de même que le Prof. Pfaff n'a peut-être pas reçu celles que je lui adressai par la meme voye) d'envoyer par le moyen des diligences, ou autre prompte occasion, les numeros à mesure qu'ils paroissent chaque mois, aux heritiers de Mr. André Cramer à Lindau qui se chargoient de me les faire passer à Côme par le Courier de Lindau qui passe chaque semaine à Milan. J'écrivis en consequence à ces memes Cramer par la voye de la poste, et je renfermais dans la lettre celle à vous, que je leur recommendai beaucoup: mais jusqu'à present je n'ai eu de reponse ni d'eux, ni ni de vous, ni moins aucun cahier des Annalen, et pour dire tout je ne reçois absolument rien de ce qu'on a fait et écrit en Allemagne sur la Physique, la Chymie etc., depuis plus de six mois. Je vous prie donc, Monsieur, de redresser cet inconvenience d'un maniere ou de l'autre: je desire trop de revoir les recherches et les progrès qu'on fait ches vouz dans ces sciences et particulierement dans les parties, que j'ai cultivées davantage. Ainsi donc songez, mon cher ami, à me dedommager des pertes et des chagrins que j'ai souffert jusqu'ici, et à pourvoir pour l'avenir: à ce que je reçoive vos annales regulierement et promptement chaque mois, ou à me communiquer faute de cela par des lettres les nouvelles litteraires que vous croyez devoir m'interesser. Voilà la tâche que j'ose vous imposer en m'obligeant à en remplir une égale. Je vous écrirai donc, si vous m'écrivez, et vous communiquerai le peu qui viendra à ma notice des travaux de nos Physiciens et Chimistes Italiens, et mes propres recherches, que je continue avec peu d'ardeur, à la vérité, ne me voyant pas beaucoup encouragé par le Gouvernement, qui ne me fournit pas même les moyens. Ma santé et des affaires de famille m'avant obligé de demander mon congé de la chaire de Pavie (ce que j'ai fait à regret) il ne m'a pas accordé les appointements entiers, que j'aurais pu pretendre d'ici à deux ans en vertu de la loi, qui accorde ces appointements entiers aux Professeur des Universités après 30 ans de service. Les 28 donc que j'ai employé a celles de Pavie avec tout ce que j'ai fait au dela du devoir précis les travaux extraordinaires, les decouvertes etc. ne m'ont valu ni gratification, ni rien soit à titre des expériences longues et faites, ou de celles á faire pour perfectionner mes recherches etc. On m'a accordé à paine les deux tiers du Salaire que j'avois, suivant que la loi l'accorde après 25 ans de service, et pour la reste on m'a regalé de belles phrases, qui ne sont pas même des promesses. J'ai reçu une gratification de six mille francs du Gouvernement François lorsque je fus à Paris[19] il y a plus d'un an et une medaille de l'Institut, en témoignage du cas qu'on faisait de mes découvertes; toutes les Academies et les Savants étrangers y ont de même attaché beaucoup d'importance, et se sont empressés de la montrer; on a fondé des prix, formé des Sociétés pour avancer les recherches sur le Galvanisme, c.à.d. sur cette nouvelle branche de Science, que j'ai presque créée. Par tout on s'occupe et on fait des progrès, et mon Gouvernement ne se soucie pas de les encourager ici, où l'on a fondé cette doctrine, d'encourager et d'aider par des moyens celui, qui de lui seul l'a portée si loin. Je suis, avec toute l'estime et l'amitié parfaite votre très humble serviteur,

VOLTA.

A Monsieur Gilbert,[20]
Professor à Halle.

[19] All this was done at the instigation of Napoléon, who had invited Volta to Paris in 1801. Arago says in his "Eloge de Alexandre Volta" (*Œuvres de François Arago*, vol. I., p. 223): "The professor from Pavie had become for Napoléon the embodiment of genius."

[20] Ludwig Wilhelm Gilbert (1769-1824), German physician and editor of the famous *Annalen der Physik und Chymie.*

II. Priestley and Lavoisier.

The documents so far discussed have either been directly connected with John Dalton, or have related to the period of chemistry influenced by his theories. The Daltonian era follows the memorable epoch in which the Phlogiston theory was overthrown by Lavoisier. This theory itself had a long reign, lasting for more than a century after it had been expounded by Becher in 1670, and later amplified by Stahl. The theory is nowadays often ridiculed, but it has nevertheless had a great, and, on the whole, wholesome influence on chemistry. Chemistry was, towards the end of the seventeenth century, only slowly emerging from the alchemistic labyrinth, which was succeeded by the iatrochemistic stage. Originally the companion of philosophers, later the handmaid of physicians, it finally entered upon an independent life. At this period the introduction into chemistry of great, far-reaching ideas was of the utmost importance. To find the laws underlying the processes, as we say nowadays, of oxidation and reduction, must be reckoned no small advance.[21] This step was made under the auspices of the Phlogiston theory, and the man who did the most important work in the field of pneumatic chemistry was himself to the end a staunch supporter of this theory. To this man, Joseph Priestley, and his great adversary in theory, Lavoisier, the documents hereinafter communicated relate.

[21] The best contribution to the history of this period, which has often been misrepresented strangely by national bias (especially by Wurtz from the French and Volhard from the German point of view) is undoubtedly: 'Die Einführung der Lavoisierschen Theorie im besonderen in Deutschland,' by G. W. A. Kahlbaum und A. Hoffmann. Leipzig, 1897.

Priestley is one of the most interesting figures in the history of science. To many features of his life's story[22] parallels can be found in Dalton's life, but in character these two great men were as different as possible. Joseph Priestley was born on the 13th March (old style), 1733, at Fieldhead, near Leeds. His father was a maker and dresser of woollen cloth, living in very straitened circumstances. Dalton's father was also a handloom weaver.

In 1742 Priestley was adopted by his father's sister. He entered the ministry in 1755, obtaining a post at Needham Market, in Suffolk, which carried with it the munificent remuneration of £30 per annum. In 1758 he accepted a post as minister at Nantwich. He tried with little success to increase his means by lectures on the use of globes. We see him here, like Dalton, as a lecturer on science at a village school. He remained three years in Nantwich. Also, like Dalton, he wrote an English Grammar.[23] In 1761 he was appointed a tutor at War-

[22] *Vide:* 'Memoirs of Dr. Joseph Priestley, written by himself,' etc. Two volumes. London, 1806-7. A 'Centenary Edition' was published in 1904. (H. R. Allenson, London.) Further, 'Joseph Priestley,' by T. E. Thorpe. London, 1906. Rutt's 'Life and Letters of Joseph Priestley.' Two volumes. London, 1832, and 'Scientific Correspondence of Joseph Priestley,' by H. C. Bolton. Privately printed. New York, 1892. Finally, the excellent article on Priestley in 'The National Dictionary of Biography.' Volume 46.

[23] Dalton's 'Elements of English Grammar, or a New System of Grammatical Instruction' (London and Manchester), 1801, foreshadows not only by the wording of its title 'The New System of Chemical Philosophy,' it shows a clear line of thought and a truly scientific inclination for uniting otherwise divergent subjects. Though dedicated to the revolutionary Horne Tooke, it is as dry as possible. Not many samples from authors are given, but the book is certainly a witness that more reading was done by Dalton than is usually supposed. Sir Henry Roscoe mentions ('John Dalton,' London, 1901, p. 188) the great chemist's statement, 'I could carry all the books I ever had on my head.' This has certainly to be understood *cum grano salis*, as is shown by the catalogue of the sale of Dalton's property, after his death, by auction, which includes a respectable library.

Reproduction of oil painting of Joseph Priestley.

rington Academy, a situation which he occupied for six years. In this period he got married. On *Pl. VII.* is reproduced a portrait which was taken at this time, and is the oldest likeness extant.[24]

Living in London, he became acquainted with Benjamin Franklin and Canton, and was led to the subject of experimental philosophy more than before. Franklin induced him to write his "History of Electricity," which is a very interesting, though somewhat one-sided book.

Dr. Percival, of Manchester, procured Priestley the title of "Doctor of Law" from Edinburgh. Dalton also, by the way, was a Doctor of Law, though of Oxford.

In September, 1767, Priestley removed to Leeds, to become minister of Mill Hill Chapel. Although at Leeds he was occupied chiefly with speculative theology, an interest in chemistry had been incited in him through a course of lectures by Dr. Turner, at Warrington. In the main he was self-taught, and, like Dalton, preferred to carry on experiments with a self-manufactured apparatus.

In 1774 Priestley obtained the position of literary companion to Lord Shelbourne. The oldest letter I have bears this year's date. Its contents are :—

DEAR SIR,

I wonder that I do not hear from you, and I find that you have received the remainder of the volume

Priestley's 'Rudiments of English Grammar,' 3rd edition, London, 1772, is also very characteristic. A chatty preface, with some interesting remarks on the usefulness of 'Academies,' 53 somewhat superficial pages, being the grammar proper, and more than three times this space in very motley notes, with examples drawn from Johnson, Hume, Smollett, Swift, Blackstone, etc., form a really interesting volume. Both books must strike the reader by the great self-assertion of the authors in fields which were outside their ordinary domains.

[24] A list of portraits of Priestley is given by Bolton, l.c., page 173—194, and by J. Yates, 'Memorials of Dr. Priestley'; 'The Christian Recorder,' 1863. The latter publication is ornamented with a book-plate of Priestley, differing somewhat from the plate in my collection, reproduced on *Pl. XV.B.*

of Institutes. There will be much more room for censure than in the former part, but I hope soon to have your finest sentiments about it

Dr. Kippis[25] has undertaken to review the Repository,[26] and thinks it will gratify the public and facilitate the revival of the work if the principal writers will allow him to give their names. I hope that you, who are *the* principal writer, will not refuse yours. Your pieces are unquestionably the most valuable in the whole work, and cannot but do you great credit.

My discourse on giving the Lord's Supper to children[27] is now published. I will send you a copy by my wife, who will be with me on Monday next, and after staying about a week, will go to Leeds, whither I should be very happy indeed to accompany her, but I am afraid it will not be possible. Nothing would give me more pleasure than to see you once more just as I used to do.

I have been of late more fortunate than ever in my philosophical pursuits, and now intend to publish an account of all I have done about air, in a separate volume, immediately.[28]

The Dissenting Committee waver much about their application to Parliament. Some of them have been influenced by courtiers. What they will do I cannot tell, nor do any of them know. This delay is very painful.[29]

[25] Andrew Kippis (1725—1795), Nonconformist, divine, and biographer. Prepared part of 'Biographia Britannica.'

[26] 'The Theological Repository,' consisting of original essays, hints, queries, etc., calculated to promote religious knowledge. Three volumes.

[27] Address to Protestant Dissenters on the subject of 'Giving the Lord's Supper to Children.' (1773.)

[28] 'Experiments and Observations on Different Kinds of Air.' London, 1774. It is often erroneously stated (by Thorpe, Kahlbaum, etc.) that the volume was only published in 1775. It was reprinted in the latter year.

[29] Not before 1779 an Act was passed permitting the dissenting ministers to preach, provided they made a declaration of belief in the Scriptures as containing the revealed will of God.

Mr. Lindsey[30] I see almost every day. He and Mrs. Lindsey are both in good health and spirits. His liturgy is almost ready for the press, but we have not yet got him a place of worship. I am afraid we shall find great difficulty in it. Sir John Pringle[31] and many of his acquaintances are very cool now that the thing is going to be put into execution, being ashamed to appear in it. They take great pains to dissuade him from it, but he is really inflexible. I have seen the York paper, and we ascribe the excellent answer to Erasmus[32] to you.

With my most respectful compliments to Mrs. Turner, and love to your boys, I am,

Dear Sir,

Yours most sincerely,

J. PRIESTLEY.

London, 19th February, 1774.

My respects to Mr. James Milner. I am promoting a subscription among my friends to defray Mr. Lindsey's necessary expenses in hiring a place of worship and the things requisite to his entering upon his scheme. Probably Mr. Milner will be pleased to be told of it and choose to contribute to it.

[This letter bears no address, but is, as its contents show, addressed to William Turner.]

30 Theophilus Lindsey (1723—1808) resigned the vicarage of Catterick (Yorkshire) in November, 1773, came to London and opened a place of worship on April 17th, 1774, in Essex Street, devoted entirely to Unitarian principles. Later on he became the preacher and owner of the first Unitarian Church, opened in Essex Street in 1778.

31 Sir John Pringle (1702—1782), Physician and President of the Royal Society. His religious convictions apparently attracted him towards the Unitarian movement.

32 William Turner (1714-1794), dissenting divine, answered, under the *nom de plume* 'Erasmus,' some attacks on Lindsey, by Dr. W. Cooper, a dignitary of the Cathedral at York.

It might appear perhaps superfluous to give *in toto* a letter dealing chiefly with religious writings. The only object is to show that Priestley was a preacher and theologian in the first instance, and only in the second instance a natural philosopher.

The year 1774 is the one in which he discovered ammonia, and it is probably to this most important discovery that he refers in the little sentence about his philosophical pursuits sandwiched between all the communications on religious subjects.

The second letter which I have is quite similar to the one communicated above. It is dated London, 10/1/1775, and—in four pages taken up almost entirely with religious topics—there is just this one sentence :—

"A new edition of my treatise on air is in the press, and I have made so many additions to my observations that I propose to publish a supplement to that work before I leave London. But the more I do, the more I see is to be done."

The various volumes of his "Experiments and Observations on different kinds of air," the work referred to here and in the previous letter, were published in the years 1774, 1775, 1777, 1779, 1780 and 1786. This work, of which Davy[33] said "that he knew no book so likely to lead a student into the path of discovery as Dr. Priestley's six volumes on air," is now very rare, at least in its original edition. It was reprinted in 3 volumes in 1790.

The departure from London refers to a journey which Priestley made with Lord Shelbourne to Flanders, Holland, Germany and Paris.

In the year 1780 Lord Shelbourne intimated to Priestley that he had no further need for his services. Priestley consequently took up his residence in Birmingham, where he became a minister of the so-called New Meeting.

[33] Davy's 'Collected Works,' vol. vi., p. 117.

From a letter written in 1782 to Joseph Banks, the famous president of the Royal Society, I give below an extract showing how much Priestley still adhered to the Phlogiston theory, on which Lavoisier had already made vigorous attacks.[34]

BIRMINGHAM,
28th Dec., 1782.

DEAR SIR,

I think myself much honoured by your sending me the Derbyshire mineral,[35] and shall endeavour, in due time, to give you the best account that I can of it. At present I have made only one experiment upon it, but this seems to afford sufficient data for explaining the phenomenon you mention.

It yields, I find, a considerable quantity of very pure or dephlogisticated air[36] by heat. Supposing, therefore, that the air incorporates with it, and thereby loses its fluidity, heat will be generated, as when water incorporates with lime, and the pure air, which that heat expels from it, will contribute to promote the escape of its phlogiston, and so produce a proper ascension,[37] as is the case with substances that contain nitre, or anything else that yields pure air with heat. I should think it is very possible to make artificial

[34] The theory was for the first time clearly set out in these two papers: 'Sur la Combustion des Chandles dans l'air atmosphérique, et dans l'air éminément respirable,' and 'Sur la Combustion en général,' both read in 1777 and printed in 1780.

[35] This was undoubtedly pyrolousite. See 'The Examination of Manganese in Experiments,' 2nd ed., vol. iii., p. 154.

[36] This is oxygen in the terminology of the phlogiston theory.

[37] Ascension means distillation in the terminology of the time, the word still lingering from the alchemistic period. Example:

'For two of our inferior works are at fixation,
'A third is in ascension . . . .'

'The Alchemist,' by Ben Johnson. It has nothing to do with decrease of weight caused by the introduction of phlogiston, as Bolton, l.c., p. 46, suggests.

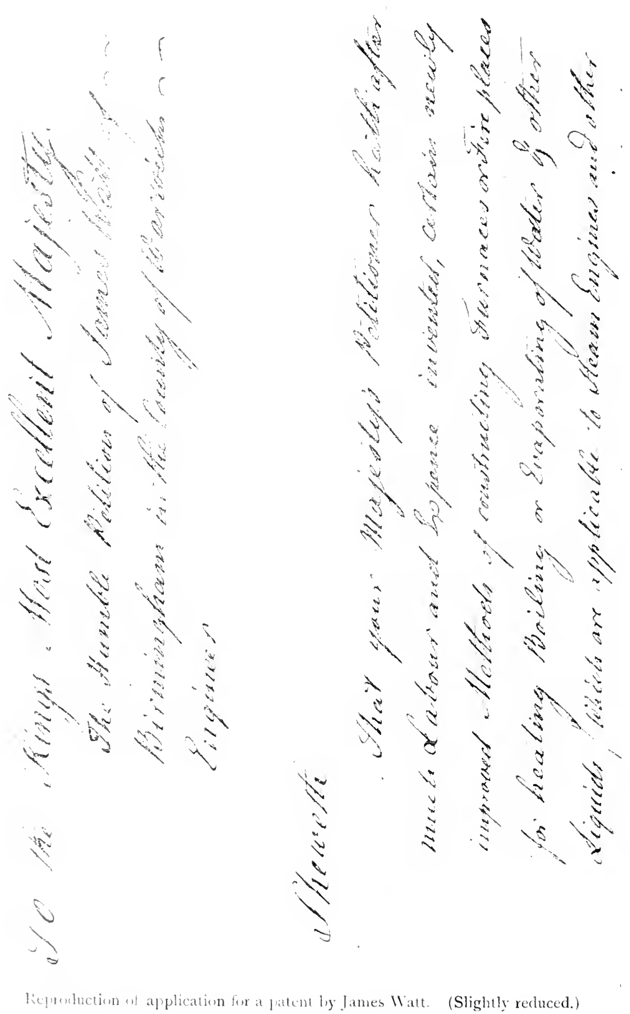
mixtures of this kind, that should have the same property in any required degree, and it is very easy to conceive that a most dangerous use might be made of them. But whatever is capable of doing mischief is likewise of doing good. . . .

JOSEPH PRIESTLEY.

It is rather difficult to understand the part of this letter, which relates to the theoretical question. It shows what difficulties the phlogiston theory had to encounter if it tried to explain the presence of oxygen in substances "which for anything that appeared had always been in the bowels of the earth." The last part of the letter is interesting, as it shows how soon Priestley thought of technical use of oxygen, a point to which he recurs at various times in his writings.

Birmingham and its surroundings was the seat of a great number of men of learning and of prominent literary merits, among whom may be mentioned Erasmus Darwin (the grandfather of Charles Darwin, a peculiar but certainly very remarkable man), James Watt, and his partner, Matthew Boulton, William Murdock, the inventor of the system of lighting by gas, Thomas Day, the eccentric author of 'Sandford and Merton,' Richard Lovell Edgeworth (whose daughter, Maria Edgeworth, is well known as the author of novels and tales for children, in some of which she collaborated with her father), who introduced into England a system of optic telegraphy. These men and several others met at the house of one of them for dinner every month, on the Monday nearest the full moon, so as to have the benefit of its light when returning home. From this they derived their name 'Lunar Society.'[38]

[38] More about the 'Lunar Society' is to be found in Bolton, l.c., pp. 195-219.


To the Kings Most Excellent Majesty.
The Humble Petition of James Watt of
Birmingham in the County of Warwick
Engineer
Sheweth
That your Majesty's Petitioner hath after
much Labour and Expense invented, certain newly
improved Methods of constructing Furnaces or Fire places
for heating Boiling or Evaporating of Water & other
Liquids, which are applicable to Steam Engines and other

Reproduction of application for a patent by James Watt. (Slightly reduced.)

I have mentioned Watt as being one of the members of the Lunar Society, and I have therefore given the reproduction of Watt's description of one of his patents, written and signed by him. (See *Pl. VIII.*) Watt's investigations into the composition of water were doubtless prompted by Priestley's communication of his discoveries to him at the Lunar Society meetings.[39]

Priestley played the part of the simple experimental philosopher, and left the conclusions to be drawn by others. He himself describes the rôle which fortune had allotted to him in the following letter:—

To Sir JOSEPH BANKS.

BIRMINGHAM,

23rd June, 1783.

DEAR SIR,

I certainly meant to submit my paper to the Royal Society, as it contains a series of remarkably new facts, completely ascertained; whatever DEDUCTIONS (about which I am not solicitous) be drawn from them.[40] As I have opportunity, I shall

[39] The literature on the somewhat difficult question concerning the discovery of the composition of water is ample. Amongst the most important contributions are: Muirhead's 'Correspondence of the late James Watt on his discovery of the composition of water.' London, 1846. G. Wilson's 'Life of Cavendish.' London, 1851. Pp. 265—445. Kopp's 'Beiträge,' part iii., p. 237. Braunschweig, 1846. M. Berthelot's 'La Révolution Chimique,' pp. 109—133. Paris, 1902. (2nd ed.) Sir Edward Thorpe's 'Essays in Historical Chemistry.' (3rd ed.) London, 1911. Pp. 79—122.

[40] 'Dr. Priestley was a discoverer before he was a chemist. In a letter which I received from him a few months before his death, he makes this statement in his usual unaffected manner. It is easy, therefore, to find reason for the occasional uncorrectness of his views. Throughout the whole course of his life his attention was never undivided. His mornings were devoted to experiment; his evenings to political, theological, or metaphysical inquiries. He is an example of how much can be done by small means, when applied with industry and ingenuity . . . .' Davy in his 'Collected Works,' vol. vii., p. 118.

prosecute the experiments farther, and if anything materially new should occur to me, I shall send you a supplemental paper on the subject.

Mr. Watt wishes to withdraw his paper, but he is now engaged in a course of experiments, in which he thinks he shall prove the actual conversion of water into air, though mine certainly prove no such thing.

When I see the young man who made the air-gun, I shall mention to him your desire of having it. It is very generous in you, and worthy of a President of the Royal Society, to interest yourself, as you do, in all scientific pursuits, however foreign to your own. It is a wide and noble field that we are employed in, and the truly liberal will rejoice in, and promote, each other's success.

I thank you for your intelligence from Paris. For my own part, I wish to see either Crawford's[41] or M. Lavoisier's FACTS unexceptionally ascertained by competent witnesses.

I have just heard from Mr. Kirwan,[42] and shall write to him as soon as I have anything worth communicating. In the meantime I wish you would inform him that I have in a glazed earthenware retort got 787 ounce measures of dephlogisticated air from TWO OUNCES of purified nitre.

With the greatest respect,

I am, dear sir, yours sincerely,

J. PRIESTLEY.

Joseph Banks, Bart., Soho Square, London.

41 Adair Crawford (1748—1795), Professor of Chemistry at Woolwich. Had published in 1799 'Experiments and Observations on Animal Heat, and the Inflammation of Combustible Bodies, being an attempt to resolve these phenomena into a general law of nature.'

42 Richard Kirwan (1733—1812). Famous chemist. Adversary of Lavoisier. His 'Essay on Phlogiston' (1779) was translated into French by Madame Lavoisier.

ETRURIA, 18*th January*, 1790.

AFTER an unremitting attention of nearly forty years to a manufactory which I have had the happineſs to eſtabliſh, and to ſee flouriſh even beyond my moſt ſanguine expectations, a wiſh to enjoy that eaſe and relaxation from the ſeverity of buſineſs, ſo neceſſary in advanced years, might perhaps meet with your indulgence: But a ſtronger motive urges me to the new arrangement which I have now the honour to acquaint you with.—I have ſons grown up, and prepared to enter into the active ſcenes of life; and a nephew, who has long conducted the buſineſs of my warehouſe in London to my entire ſatisfaction. They have chearfully undertaken to unite their beſt endeavours in carrying on the various branches of this manufactory, and promiſe to purſue, with alacrity and diligence, the improvements which I have begun. I have therefore aſſociated them with me in buſineſs, under the firm of Joſiah Wedgwood, Sons, *and* Byerley.

Permit me to take this opportunity of returning you my ſincere thanks for the favours you have been pleaſed to confer upon me, and to entreat the continuance, to this new eſtabliſhment, of that goodneſs which I have ſo long experienced, aſſuring yourſelf of our utmoſt endeavours to merit your friendſhip and eſteem.

I have the honour to be, with the greateſt regard,

Your much obliged,

and moſt obedient

humble Servant,

Josiah Wedgwood

Business circular of Josiah Wedgwood.

The famous controversy concerning the composition of water between Watt and Priestley is referred to in one of the sentences of this letter. Watt showed himself the better reasoner, whereas Priestley was the greater experimentator. It is significant that in the same letter Lavoisier's name is mentioned. I think the comparison between Watt's work and Priestley's can be made with equal justice with regard to Lavoisier and Priestley. I shall, however, have occasion to discuss Lavoisier's work later on, and will therefore proceed to the connection of Priestley with another man.

Josiah Wedgwood was a frequent guest of the Lunar Society, but Priestley had already been on friendly terms with him before going to Birmingham.[43]

Amongst his autographs in my possession there is one which I reproduce on *Pl. IX.* This is the circular which Wedgwood sent to his customers announcing that he had taken his son into partnership. Wedgwood was not only an admirer of Priestley, but also a strenuous helper in supplying him with his excellent earthenware materials, which were then, as for many years after, highly valued for scientific research work.[44] In return for his help, Priestley straightway communicated to him all his discoveries. This gave great satisfaction to Wedgwood, who was himself as prominent in science as in artistic handicraft. Among my letters is one addressed to Wedgwood and endorsed by the latter :—

May, 1785.

DEAR SIR,

About the time that this comes to hand I hope you will receive three copies of my paper of experi-

[43] See 'Life of Josiah Wedgwood,' by E. Meteyard. London, 1865-66, vol. i., p. 391.

[44] See 'Life,' vol. xi., p. 557 ; also Priestley's 'Memoirs.' Cent. ed., p. 60.

ments printed for the Philosophical Transactions, of which one is for yourself, another for Dr. Darwin, and the third for the gentleman who was so obliging as to join you in contributing to the expense of my experiments but wished to be unknown.[45]

I am making the most of the fine sunshine we now enjoy, and have lately discovered some very remarkable new facts, which promise to throw much new light on the doctrine of air, &c. They could not be made but by means of a burning lens.

I have been just trying a new process for procuring the charcoals of the several metals, some of which I shewed you, but it has not yet succeeded; but I do not despair, and I hope to do this and much more, when I get a larger lens. As soon as I can get a tolerable assortment of these new modifications of the metals, I shall send them to you, either in London, or at Etruria.

I wish your business of application to Parliament was in as good a train as my experiments are at present. Your exertion, tho' unsuccessful, will do you the greatest honour.

With my respectful compliments to Mrs Wedgwood and your son, I am, Dear Sir,

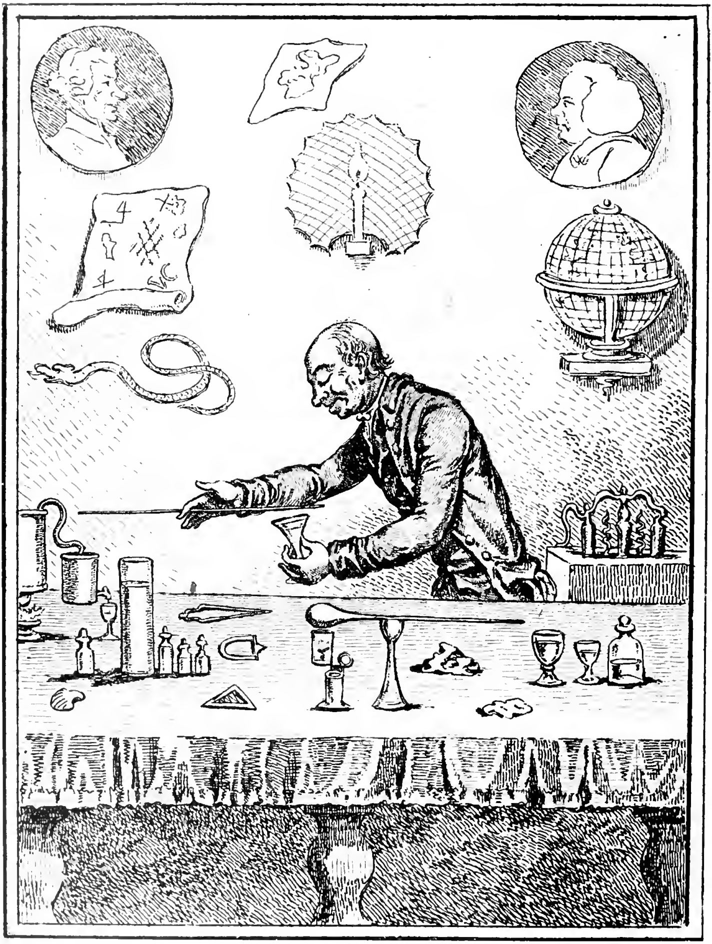
Yours sincerely,

J. PRIESTLEY.

The use of lenses for the production of high temperatures was very common. It allowed very clean work, and the disturbing influence of the burning of coal as a means of involuntary reduction was eliminated. The parliamentary business of Wedgwood, to which allusion is

[45] About the many people who assisted Priestley by yearly allowances or occasional gifts *vide* 'Memoirs,' p. 59. Priestly mentions in his 'Appeal to the Public on the subject of the Riots in Birmingham' (1791), part ii., p. 105, that the Philosophical Society of Manchester had made him a member and granted him £50 'to assist me in defraying the costs of my experiments.'

Priestley caricature. (Original size.)



Priestley caricature. (Slightly reduced.)

made, was an effort to oppose the proposals of the Government to free Ireland from the commercial restrictions which had been imposed upon her since the Revolution. Wedgwood was unsuccessful in his opposition to the Government.[46]

Priestley, too, soon found himself at variance with the Government, and his very life was endangered, as we shall see.

In November, 1790, Edmund Burke had published his 'Reflections on the French Revolution,' and our philosopher, who believed in the doctrines of Thomas Paine, the author of 'The Rights of Man,' answered Burke in a pamphlet, which favoured the French revolutionists.

Burke replied in the House of Commons, and Priestley thus acquired the reputation of a dangerous character. On *Pl. X.* can be found a contemporaneous caricature. Priestley is seen here with his foot on a book on which is written, 'The Bible explained away.'

This refers to his doctrines, in which he attributed such a power in religious matters to reason, that he had fallen into disfavour with the Established Church.

He lost all credit with the King and Church Party, and was certainly one of the most unpopular men of the day, especially as the Dissenters endeavoured to obtain a repeal of the Test Act. As a theologian he was ridiculed, as the caricature shows. Even his more peaceful occupation was the subject of the caricaturist's satire. (*Pl. XI.*)

Whatever his true or imaginary faults and mistakes may have been, the punishment which followed was excessive.

On the 14th July, 1790, the signal for the French Revolution had been given by the capture of the Bastille. In the following year this memorable event was to be

46 See 'Life of Josiah Wedgwood,' vol. xi., p. 535—36.

celebrated by a dinner at Birmingham. It must be confessed that Priestley had done much to excite the wrath of his opponents. He had published pamphlets when he ought to have kept the peace, and although a most seditious handbill, which had been distributed broadcast early in July, had not been written by himself, nor even influenced by his party, the catastrophe came on the anniversary of the capture of the Bastille.

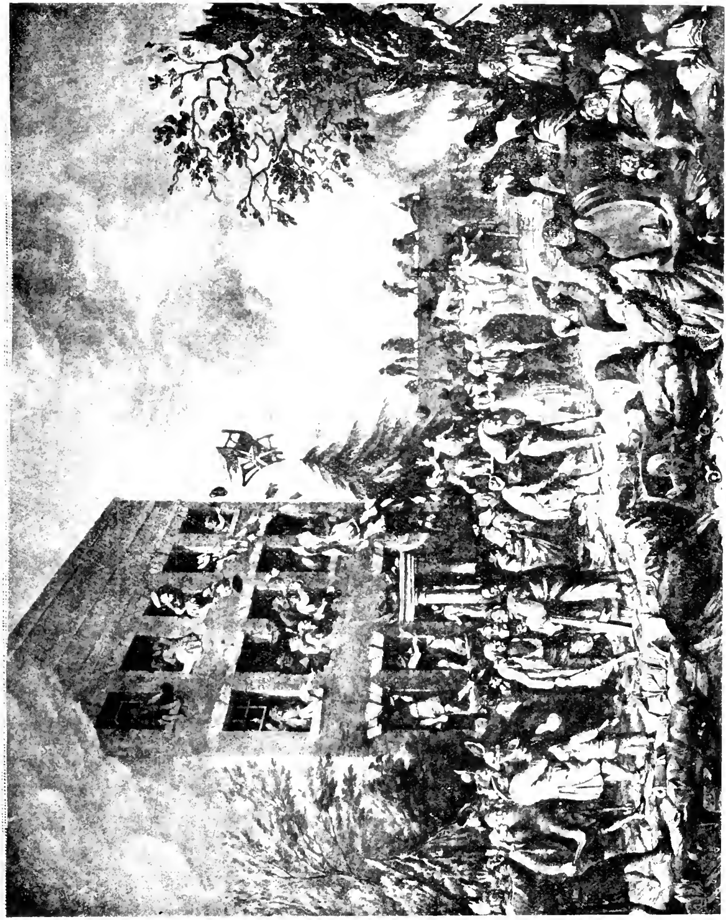
A crowd of rowdies assembled near the hotel where the dinner was to take place and broke every window. Priestley, by the way, was not present himself. After this little preamble the crowd hurried to the New Meeting House, where Priestley used to minister. This house was pillaged and burnt, as also was a second Meeting House.[47]

After the riots Priestley went to London, where he stayed first at Tottenham and later on at Hackney. I possess two letters of Priestley written in reference to his troubles. The first is addressed to Josiah Rees, a Welsh Presbyterian minister.

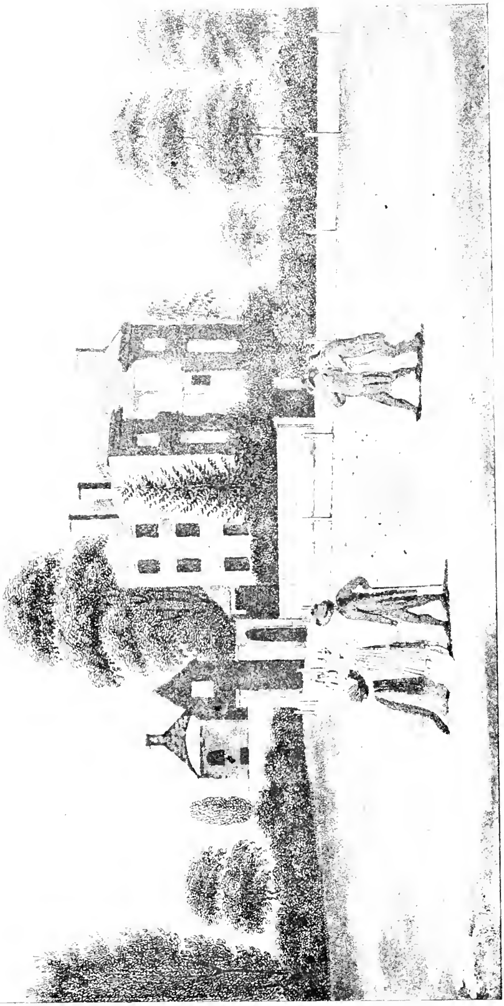
REVEREND SIR,

A variety of engagements and absences from London have prevented my noticing your very obliging letter so soon as I should otherwise have done. The first opportunity that you have, I beg you would assure the ministers in whose name you wrote to me, that I received the greatest satisfaction from their consolatory letter, and that I shall be far from considering my sufferings as a cause of lamentation, if they be the means, as I trust they will, of leading the Dissenters of different persuasions to feel for one

47 See 'An Authentic Account of the Riots in Birmingham.' Birmingham, 1791. The second edition of this pamphlet contains an appendix: 'The Claims of the Sufferers and the Verdicts of the Juries.' See also the chapter, 'The Birmingham Riots,' in Thorpe's 'Joseph Priestley,' and the 'Appeal,' mentioned in note 45.



The Birmingham Riots. Destruction of Priestley's house.



Priestley's house in ruins.

another as brethren, and to value as they ought the great principles that are common to us all, and these we shall find are of infinitely more importance than all those about which we differ. Those great doctrines which lead us to acknowledge the providence of God here, and to look into futurity, so as to enable us to bear as we ought both the good and the evil of this life, we all agree in, as well as in acknowledging but one lawgiver in the church, Jesus Christ, and a submission in all events to his authority, in opposition to every thing human.

We have much to fear from the rising spirit of bigotry, encouraged by the clergy, and I fear the Court too, but with Christian prudence and fortitude we shall certainly overcome.

With very respectful compliments to your brethren and yourself, I am, Rev. Sir,

Your very humble servant,

J. Priestley.

London, Oct. 3, 1791.

The second letter, which is addressed to Joseph Banks, is reproduced on *Pl. XIV.*

I believe that, in all its brevity, it is the most touching of all the documents we have concerning the cruel condition of the 'broken philosopher.'

Priestley set up a new laboratory at Hackney, and on the back of the letter which is reproduced on *Pl. XV.A* are the remarks of Sir Joseph Banks, enumerating the things with which he intends to provide his friend.

It must by no means be imagined that the excesses of the Birmingham mob had sobered his enemies or brought the least semblance of shame to their minds.

In the *Times* of 14th July, 1792, one year after the riots, there appeared an abominable article, celebrating the anniversary of the 'Glorious Birmingham lamp-lighting.'

He was often, together with Thomas Paine, burnt in effigy, and the King and Church party did everything in their power to make his existence impossible in England.

The only comfort came from his scientific friends, who forgot whatever quarrels they had with him, and offered their help. In 1790 he had had a heated exchange of letters with Banks about a certain Mr. Cooper, whom he had recommended to the Royal Society, but who had been refused admission. In one of the letters in my possession relating to this quarrel he says :—

To Sir Joseph Banks.

72 ST. PAULS, 25 April, 1790.

DEAR SIR,

I cannot forbear to express my great dissatisfaction at the conduct of the Royal Society in the rejection of Mr. Cooper, recommended by myself and four other members.

I consider this business as the effort of party spirit, political or religious, highly unworthy of the Society, injurious to the interests of philosophy, and arising from principles which would equally lead to my own exclusion from the Society. . . .

JOSEPH PRIESTLEY.

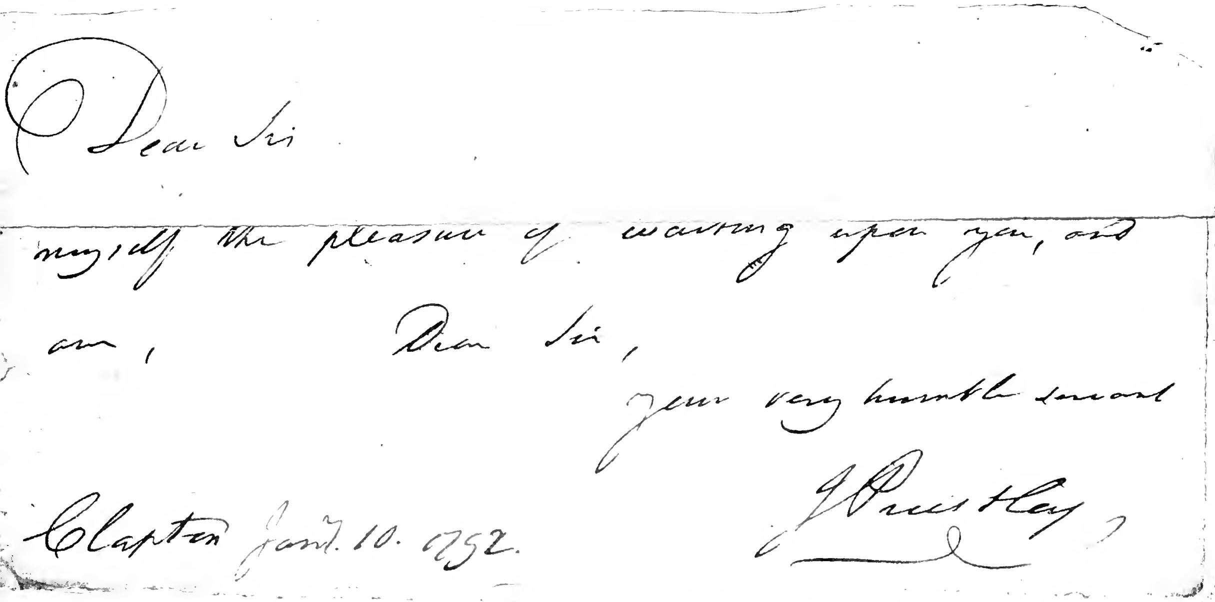
To Sir Joseph Banks.

72 ST. PAULS, 27 April, 1790.

DEAR SIR,

You say that 'no token of Mr. Cooper's scientific merit has hitherto been brought forward to the Society.'[48] But is this the case of more than perhaps

48 'The becoming of F.R.S. was at the time entirely dependent upon Sir Joseph Banks' goodwill.' The candidate was generally presented to Sir Joseph at one of his Thursday morning breakfasts, and, if unobjectionable, was introduced to the influential and leading fellows. The view which Sir Joseph took of the constitution of the Royal Society was, 'that it shall consist of two classes—the working men of science and those who, from


Dear Sir

myself the pleasure of waiting upon you, and

am, Dear Sir,

your very humble servant

J Priestley

Clapton Jany. 10. 1792.

Reproduction of Priestley's letter to Sir Joseph Banks (Original size.)

Dear Sir

Having lost my whole stock of substances, ores, minerals, earths &c &c for the purpose of experiments, and being willing to replace them as expeditiously as possible, I shall be obliged to you if you will mention my situation to any of your friends whose laboratories are furnished, and who may have any thing to spare to set up a broken philosopher.

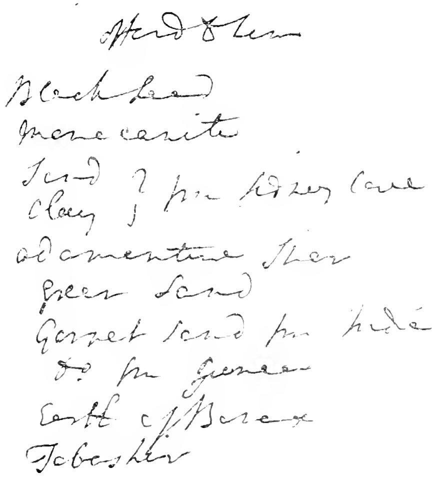
I shall take the first opportunity of doing myself the pleasure of waiting upon you, and am, Dear Sir,

your very humble servant

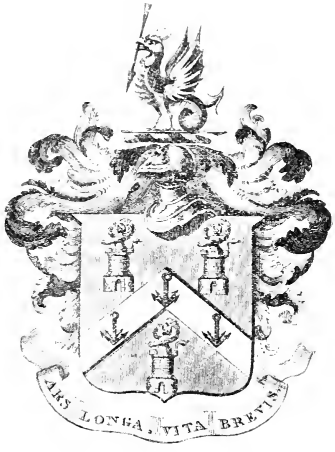
J Priestley

Clapton Jan^y. 10. 1792.

Reproduction of Priestley's Letter to Sir Joseph Banks. (Original size.)


Afford them
Black lead
menacanite
Sand } from [illegible] Cave
Clay }
adamantine Spar
Green Sand
Garnet Sand from Inde
do. from Genoa
Earth of Borax
Tabasheer

A. Reproduction of note by Sir Joseph Banks on the back of his letter. (*Pl. XIV.*)



B. Priestley's bookplate.

one in ten of the members, especially of gentlemen of fortune, and liberal education, like Mr. Cooper? . . .

JOSEPH PRIESTLEY.

Banks, of course, thrust aside all thoughts of ill-feeling when he saw Priestley in this hapless condition, and hastened to his aid.

I have, however, another document, also in connection with the help offered at this time to Priestley, which is still more remarkable.

Its full contents are:—

Les Chimistes de Paris au Docteur Priestley, Salut.

A la nouvelle des dangers que vous aves courus et des foureurs auxquelles vous aves échappé, tous les Etudiants en Chimie, en médicine et en Pharmacie, se sont réunies pour vous adresser l'hommage de leur sensibilité: c'étaient vos disciples qui se rassemblaient et tous ceux qui s'occupent des sciences dans cette Capitale se sont fait gloire de se ranger parmi eux.

Vous aves, Monsieur, ouvert dans les Sciences des routes nouvelles: vous avés honoré le siecle et le pays qui vous avont vû naitre; vous a avés été sensible, bon vertueux et juste; il manquait à votre gloire d'avoir été persécuté: et c'est un nouveau point de ressemblance que vous vénés d'acquérir avec les philosophes les plus célèbres de l'antiquité.

Comme Citoyen vous appartenés à l'Angleterre, et c'est à elle à réparer vos pertes[49]: comme savant vous appartenés surtout à ceux qui savent vous apprécier;

their position in society or fortune, it might be desirable to retain as patrons of science.' From Weld's 'History of the Royal Society.' London, 1848, vol. ii., pp. 152—153. The special case here alluded to by Priestley is fully discussed in his 'Appeal to the Public on the Subject of the Riots in Birmingham.' (2nd ed.) 1792. Part 2, p. 106.

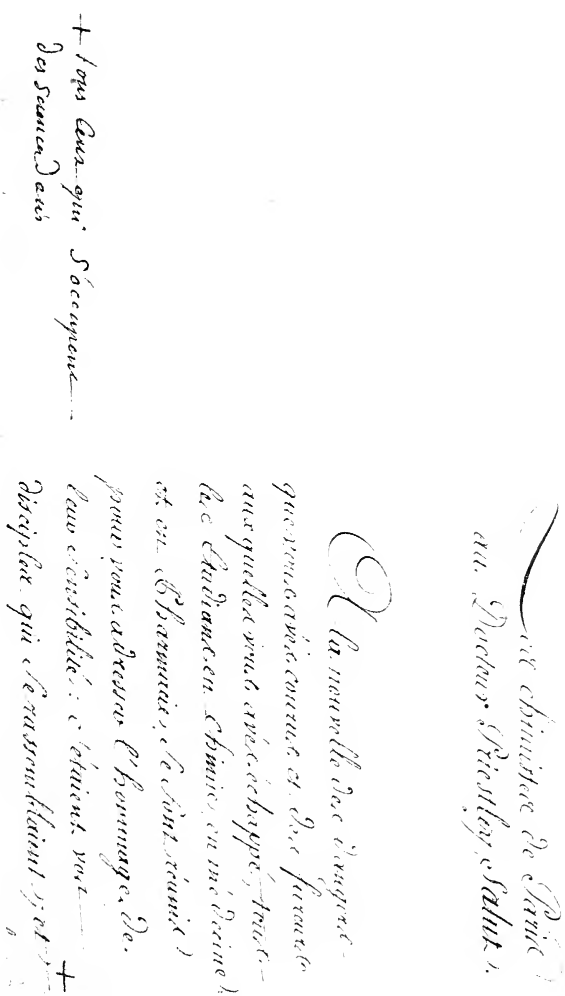
[49] Priestley claimed originally damages for £4,112. 16s. 9d.; his claim in court amounted to £3,628. 8s. 9d., and he was allowed £2,502. 18s. ('An Account,' etc. 2nd ed., p. 36.)

et c'est nous, sous ce rapport, qui devons vous restituer les mesmes instruments que vous aves employes si utilement a nous instruir. Nous avons donc résolu de rétablir votre Cabinet, de relever ce Temple que l'ignorance, la barbarie et la superstition ont osé prophaner. Quel service plus important pourrions nous rendre aux sciences que de vous mettre en mains les instruments nécessaires pour les cultiver?

Deffenseurs zelés de la liberté que notre pays vient de conquérir, nous n'avons pas été des temoins oisifs de la plus étonnante des revolutions, dont les annales du monde ayent conservé le souvenir. Nous en avons suivi les mouvements et les progrès avec cet esprit d'observation que donne l'étude des sciences et que nous avons puisé dans les ouvrages de nos maitres. Si au milieu de cet enthousiasme civique que nous avons tant de fois partagé, le peuple français a pu se porter quelquefois à des des excès coupables, c'est toujours contre l'oppression qu'iletait armé: ildeffandait la caise de la tolerance, de la liberté et de la philosophie. On ne se serait pas attendu qu'un peuple, qui se dit libre et quie se croit éclairé se fût porté à des excès en sens opposé. Nous nous garderons de l'en accuser: les vrais coupables sont ceux qui l'ont égaré. Nous imiterons vos vertus et nous répéterons avec vous que les excès même qui ont été commis, ont plus fait en quatre jours por les progrès de la raison, de la tolérance et de la philosophie, que les écrits des hommes sages n'en auraient pu faire en deux siècles.[50]

The importance of the document lies in the fact that the various corrections, of which one can be seen on the left hand side, are written by Lavoisier. (See *Pl. XVI.*)

[50] This is an unsigned draft. It is doubtful if the letter was eventually sent to or received by Priestley. It is not mentioned in the list of addresses which Priestley received after the riots as stated in the preface of his 'Appeal' on pages 26 and 27, where twenty-two addresses are enumerated.


Les Chimistes de Paris
au Docteur Priestley, Salut.
La nouvelle des dangers
que vous avez courus et des fureurs
auxquelles vous avez échappé, tous
les Etudiants en Chimie, en médecine
et en Pharmacie, se sont réunis
pour vous adresser l'hommage de
leur sensibilité : c'étaient vos
disciples qui se rassemblaient ; et +

+ tous ceux qui s'occupent
des Sciences dans

Draft of letter of the French chemists to Priestley, with corrections in Lavoisier's handwriting. (Slightly reduced.)

It is thus evident that Lavoisier, who had many a scientific rencontre with his English contemporary,[51] showed himself a staunch friend in the hour of need.

Before entering into the details of the relations which existed between Priestley and Lavoisier, I shall, in just one or two sentences, bring the history of the former's life to an end.

In 1794 Priestley sailed, with his wife, from London to New York, where he was well received. A professorship at Philadelphia was offered to him, but refused. For ten years he lived at Northumberland, occupied until the day of his death with theological and scientific studies.

Thus, after all, his adventurous life ended peacefully.

But the great Lavoisier had met his death on the guillotine even before Priestley had reached the shores of America.

The French people, which had always stood, as Lavoisier himself wrote, on the side of tolerance, liberty, and philosophy, had sent him to the guillotine.

With the exception of the tragic finale, his life's history was simple enough.[52]

Born in 1743, the son of a respected lawyer, he had an excellent education, and became already, in 1768, "adjoint" to the French Academy. He inherited a large fortune from his mother, and grew still wealthier as a result of his marriage. Besides his scientific pursuits, he was most active in public life. He had become associated with the body known as "fermiers." These were financiers, whose duty it was to collect the state taxes, for which they paid a fixed sum to the Crown.

[51] This most unpleasant chapter has been treated fully in the works by Grimaux and Berthelot from the French point of view. Compare Sir E. Thorpe's 'Essays,' pp. 149—184.

[52] See 'Lavoisier,' by E. Grimaux. 3rd ed. Paris, 1899.

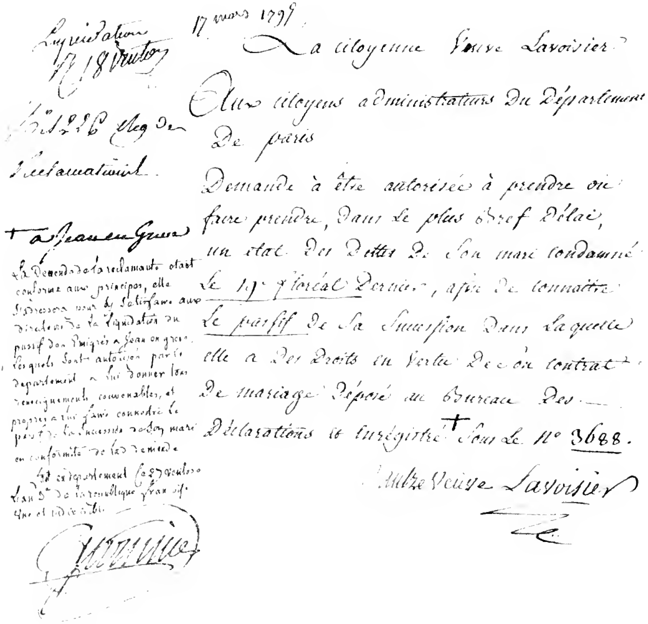
Lavoisier's experiments, referring to the measurement of specific heat, which he worked together with Laplace, are most significant. His experiments on the production of respiration are also of great importance.[53]

But Lavoisier's chief title to immortality rests, not so much on his experimental work, as on the new and most ingenious way in which he interpreted the experiments of others.

As already mentioned, the period of Priestley's best experimental work was about 1771-1779. It is no mere accident that Lavoisier's most important publications date from approximately the same period. Lavoisier was the first to see the true importance of the discovery of oxygen. He saw that this gas had to be present in all processes in which the chalx of a metal is formed from the metal itself. But he saw still more; he saw clearly that the explanation of this process which is accompanied by an increase in weight was to be found in the simple chemical addition of the weight of oxygen. These discoveries took him still a step further. In following up all his experiments exactly with a balance, Lavoisier found that, whatever processes took place, the total weight of all the chemical compounds acting in these processes was never changed; in other words, he found one of the two great propositions which, so to speak, govern the way in which we nowadays regard all processes in chemistry, namely, the conservation of matter. All this, of course, was in part exactly the opposite of the then still all-powerful theory of phlogiston; it therefore amounted to nothing less than a revolution in chemistry.

Most unfortunately, Lavoisier's life coincided with the

[53] A list of Lavoisier's publications is given in Grimaux's work, pp. 336—358. His works have been edited under the care of the Minister of Public Instruction in France. (Paris, 1846—1893.) The works of Priestley have not yet been collected.



Document signed by Madame Paulze Lavoisier, wife of Antoine Lavoisier. (Reduced.)

great political revolution. The chief facts which involved him in it were, so far as can be made out, as follows :—

He was rich, and moreover, he had once criticised somewhat severely a treatise on Heat written by the demagogue Marat. He was also a member of the hated body of the fermiers-generals.

You might, perhaps, consider that all these reasons are hardly sufficient to bring anyone to the guillotine. But nevertheless so it happened, and he had his death warrant signed on the 7th May, 1794, and was guillotined on the following day. The most significant words which the brute of a judge (called Coffinhal) threw into the face of the great Lavoisier were, 'The Republic does not need any scientists.'

Madame Paulze Lavoisier had courageously supported her husband's cause throughout the five months of his trial. Even after his execution she continued the struggle and finally succeeded in obtaining the reconstitution of his property.[54]

I have a document, written by her some time after her husband's death, and which relates to matters of his personal estate. (See *Pl. XVII.*)

I hope I have at least demonstrated the fact that valuable information can be obtained from autograph documents. Especially is this the case if one extends the collection over contemporaneous pictures, pamphlets, etc. Of course, the study of history is impossible without first studying the printed books or papers of the various scientists, and autograph documents can only afford additional, though very often valuable, information. If you read Macaulay's history, you will find how often he refers

[54] She became later on the wife of Benjamin Thompson, better known as Count Rumford.

to the publications of Strype. The latter was one of the first autograph collectors of the old times, and his publications afforded most essential assistance later on to the works and publications of Macaulay. My aim is at the best not to do more than Strype has done: to prepare the way and collect the material for the historian.

PROCEEDINGS

OF

THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 1st, 1912.

The President, Professor F. E. Weiss, D.Sc., F.L.S., in the Chair.

Mr. C. L. Barnes, M.A., drew attention to the recent accessions to the Society's Library, and a vote of thanks was accorded the donors of the books upon the table. The following were amongst the recent accessions to the Society's Library: "*Russia's Policy in Finland*," by G. Evreinov, trans. from the Russian by V. E. Marsden (8vo., London, 1912), presented by the Translator; "*Results of Meteorological Observations made in New South Wales*," during 1887, 1888, 1889, 1890, 1898, 1899 and 1900, 1901 and 1902, under the direction of H. C. Russell (8vo., Sydney, 1889-1892, 1900, 1903 and 1904), "*Results of Rain, River, and Evaporation Observations made in New South Wales*, during 1888, 1889, 1890, 1891, 1892, 1893, 1894, 1895, 1896, 1897, 1898, 1899, 1900, 1901-1902, under the direction of H. C. Russell (8vo., Sydney, 1889-1904), and a collection of twenty-seven meteorological papers by H. C. Russell, presented by A. W. Waters, Esq., F.L.S., F.G.S.; "*Flora capensis*," vol. 5, sect. i., pt. iv., by Sir W. T. Thiselton-Dyer (8vo., London, 1912) purchased; "*In and Around the Morteratsch Glacier*...," by J. Y. Buchanan (8vo., n.p., 1912), presented by the Author; "*Geologic Atlas of the United States*," folios Nos. 174-182 (la. fol., Washington, D.C., 1910-1912), presented by the United States Geological

Survey. "*Lagenae of the South-West Pacific Ocean. From soundings taken by H.M.S. 'Waterwitch,'* 1895," by Henry Sidebottom (8vo., London, 1912), presented by the Auther; "*The Record of the Royal Society of London...,*" 3rd ed. (4to., London, 1912), and "*The Signatures in the First Journal-Book and the Charter-Book of the Royal Society*" (la. fol., London, 1912), presented by the Royal Society of London: "*Subject List of Works on Mineral Industries...in the Library of the Patent Office,*" Pt. 2, New Series, XN 40—XR, Pt. 3, New Series, XS—YH (16mo., London, 1912), and "*Subject List of Works on Horology...in the Library of the Patent Office,*" New Series, FO—FR (16mo., London, 1912), presented by the Patent Office, London: "*Meridian-Beobachtungen von Sternen in der Zone 65°—70° nördlicher Declination, II. Katalog für das Aequinoctium 1900.0,*" von H. Geelmuyden und J. Fr. Schroeter (fol., Christiania, 1912), presented by the Observatorium, Christiania: "*Catalogue of the Periodical Publications, including the Serial Publications of Societies and Governments in the Library of the University College, London.*" by L. Newcombe (8vo., Oxford, 1912), presented by the University College, London; "*The Arabic and Turkish Manuscripts in the Newberry Library,*" by D. B. Macdonald (8vo., Chicago, Ill., 1912), presented by the Newberry Library: "*De Temperatuur-invloed op Physiologische Processen der Alcoholgist.*"...door J. E. van Amstel (8vo., Amsterdam, 1912), "*Het Aethyleeren van Chloorbenzol,*"...door J. G. W. Sieger (8vo., Amsterdam, 1912), "*Proeve eener Theorie van het Roteerend Magnetisch Veld,*"...door P. M. Verhoeckx (8vo, 's Gravenhage, 1912), and "*Stranden en Strandverdediging,*" .. door L. R. Wentholt [Text and Atlas] (8vo., Delft, 1912), presented by the Technische Hoogeschool, Delft; "*Rapporten van de Commissie in Nederlandsch-Indië voor Oudheidkundig Onderzoek op Java en Madoera,*" 1909 and 1910 (4to., Batavia, &c., 1911), presented by the Bataviaasch Genootschap van Kunsten en Wetenschappen; "*Die Wind-verhältnisse in den oberen Luftschichten nach Ballonvisierungen in Batavia,*" von W. van Bemmelen (4to., Batavia, 1911), and I. "*Drachen- und*

Fesselballon-beobachtungen," II. "*Wissenschaftliche Ergebnisse der Aufstiege mit dem Freiballone 'Batavia'*," von C. Braak (4to., Batavia, 1912), presented by the Koninklijk Magnetisch en Meteorologisch Observatorium te Batavia; and "*Censo General de la Ciudad de la Plata*," by C. P. Salas and A. C. Alcorta (4to., La Plata, 1910), presented by the Direcciòn General de Estadistica de la Provincia de Buenos Aires.

New exchanges have been arranged with the Tôhoku Imperial University (*Science Reports* and *Tôhoku Mathematical Journal*), Sendai; and the Bureau of Productive Industries, Government of Formosa (*Icones Plantarum Formosanarum, nec non et Contributiones ad Floram Formosanam*), Taihoku.

A copy of the Address presented to the Royal Society of London, at the celebration of the 250th anniversary of its foundation, was read by the PRESIDENT. The Address was as follows:—

"Praesidi Consilio Sodalibus Societatis Regalis Pro "Scientia Naturali Promouenda annum CCL suum feliciter "celebrantis S.P.D. Societas Litteraria et Philosophica "Mancuniensis.

"Etsi uix omnibus persuadebit poeta qui censebat

"In magnis et uoluisse sat est,

"tamen cum praeclaros Societatis Vestrae annales per tot "iam saecula florentis spectemus, nostrae certe non ingrata "laus erit si quis nos uoluisse iudicauerit, quantum quidem "intra prouinciam nostram fieri posset, insistere uestigiis "Vestris. Nec sine gloria quadam propria nobis, qui usque "ad hunc diem Daltonii illius domum habitamus, recordari "licet et Daltonium ipsum et Ioulium,—quibus qua nomina "in rebus physicis illustria?—communes socios Vestri et "nostri corporis fuisse, nec non inter nos, ut inter famili-"ares suos, aliquanto prius reperta sua quemque esse "confessos. Nonne enim, ut cecinit Salomo, "ceu ferrum "ferro, sic ab amico exacuitur amici facies?" Nos certe si "quid unquam boni in medium conferre uel poterimus uel "potuimus, inde id nobis contigerit quod Vestro exemplo "instincti doctos uiros et naturae inuestigatores singulos ac "solos laborare non patimur, sed in sociorum conuiuia, in "rationis commercia attrahere conamur. Sit de nobis "quoque dictum, ut a Vergilio olim cum Roma Mantuam "comparante,

"Sic canibus catuli similes, sic matribus haedi,

"dummodo quis hoc pro certo habeat nos haedos, quan-"quam iam et ipsi per centum et triginta annorum cursum "saltauimus, uictorias ac triumphos Vestros, uelut Parentis, "summo semper gaudio reuereri, feriasque Vestras hoc "tempore laetissime celebrare. Et in tanto hoc populo, "cuius necessitates in dies acrius Scientiae exauctae opem "ante omnia implorant, diu Vobis excitare ac ducere liceat "magnum istum exercitum quarentium ueritatem.

"Horum ergo uotorum nuntium, Praesidem nostrum "dilectum, artis botanicae acerrimum Professorem, Frederi-"cum Ernestum Weiss, a Vobis benigne uocati delegauimus "qui Vobis ipse laetantibus laetitiam nostram repraesentet.

"F. E. WEISS, *Praeses.*
"R. L. TAYLOR, } *Secretarii.*
"GEORGE HICKLING, }

"Datum Mancunio
"ex aedibus Daltonianis
"Kal. Iul. MDCCCCXII."

Mr. WILLIAM BURTON, M.A., F.C.S., gave a brief account of the life and work of Mr. Alfred Brothers. Mr. Brothers was elected a member of the Society in 1860; thus, at the time of his death, on August 25th, he had for more than fifty years been a member of the Society.

The PRESIDENT exhibited and made some remarks on a specimen of the so called Crown or Mummy Pea.

Professor G. ELLIOT SMITH, M.A., M.D., F.R.S., read a paper entitled, "**Ancient Stone Monuments.**"

The paper will be printed in the *Memoirs*.

General Meeting, October 15th, 1912.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., F.C.S., Vice-President, in the Chair.

Mr. J. C. MAXWELL GARNETT, M.A. (Cantab.), Principal of the Municipal School of Technology, Manchester; Mr. MILES WALKER, M.A., M.I.E.E., Professor of Electrical Engineering, The Municipal School of Technology, Manchester, *The Cottage, Leicester Road, Hale, Altrincham;* Mr. D. M. FAIRLIE, M.Sc. (Manc.), Demonstrator in Electro-Chemistry, The Municipal School of Technology, Manchester; Mr. R. STEPHEN ADAMSON, M.A., B.Sc., Lecturer in Botany, The University, Manchester; Mr. W. B. BRIERLEY, M.Sc. (Manc.), Lecturer in Economic Botany, The University, Manchester; and Mr. JOHN MCFARLANE, M.A. (Edinb.), B.A. (Cantab.), M.Com. (Manc.), Lecturer in Geography, The University, Manchester, were elected ordinary members of the Society.

Ordinary Meeting, October 15th, 1912.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., F.C.S., Vice-President, in the Chair.

A vote of thanks was given to the donors of the books upon the table. These included volumes I.—XX. of the "*Annals of Botany*" (8vo., London, 1887-1906), presented by The President, Professor F. E. Weiss; and "*Subject List of Works on Mineral Industries...in the Library of the Patent Office*," Pt. 1, New Series, WN—XN 39 (16mo., London, 1912), presented by the Patent Office, London.

Professor G. ELLIOT SMITH, M.A., M.D., F.R.S., exhibited lantern slides of a Second Dynasty Egyptian coffin in the collection of the Manchester Museum, and made some remarks as to its bearing upon the theory of megalithic monuments presented by him at the last Meeting.

Mr. ARTHUR ADAMSON, M.Sc.Tech., A.R.C.S., **exhibited and described an apparatus which could be used for the exact trisection of an angle,** a problem which is, in general, impossible of solution by ordinary geometrical methods.

A paper by Mr. Adamson on the subject will appear in the *Memoirs*.

Professor W. W. HALDANE GEE, B.Sc., M.Sc.Tech., A.M.I.E.E., said that since Mr. Adamson explained his model to him he had taken some interest in the historic problem of the trisection of an angle. About 2,300 years ago the sophists were busy with three geometrical problems—(1) squaring the circle, (2) doubling the cube, and (3) the trisection of the angle—few problems in mathematics have been studied so persistently. They were found insoluble by the use of straight lines and circles only. Hippias of Elis was one of the first to study the trisection problem. He discovered the quadratrix. About 200 B.C. the conchoid was invented and applied to the trisection of an angle. In the works of Pappus a number of solutions are collected. Reference was also made to the work of Archimedes (287-212 B.C.), Vieta (1540-1603), Snell (1591-1626), Descartes (1596-1650), Viviani (1622-1703), Pascal (1623-1662), Huygens (1629-1695), Tschenhausen (1631-1728), Newton (1642-1727), and others.

A paper by MR. D. M. S. WATSON, M.Sc., entitled "**The larger Coal-Measure Amphibia,**" was read by Dr. HICKLING.

The paper is printed in full in the *Memoirs*.

General Meeting, October 29th, 1912.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

Mr. A. W. RYMER ROBERTS, M.A. (Cantab.), *Ellerbeck, Crook, near Kendal,* was elected an ordinary member of the Society.

Ordinary Meeting, October 29th, 1912.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

A vote of thanks was accorded the donors of the books upon the table. These included: "*A Dictionary of the Biloxi and Ofo Languages*," by J. O. Dorsey and J. R. Swanton (8vo., Washington, 1912), and "*Early Man in South America*," by Aleš Hrdlička and Others (8vo., Washington, 1912), presented by the Bureau of American Ethnology.

Mr. THOMAS KAY, J.P., exhibited sketches of the three voltaic experimental piles of Alessandro Volta—the perpendicular form (with a model of the original), another form in the dry plates, and one in wet tubes. Mr. Kay also showed a sketch of the bust of Volta in the Museum at Como, and a daguerreotype of Michael Faraday, D.C.L., and John Frederick Daniell, D.C.L., taken at Beard's Gallery in London, 1841, obtained from Dancer, the Manchester optician.

Dr. KURT LOEWENFELD read a paper entitled **"The importance of autograph documents in the History of Science" (Part I.).**

The paper will be printed in full in the *Memoirs*.

General Meeting, November 12th, 1912.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

Miss MARJORIE LINDSEY, B.Sc., Research Student in the Victoria University of Manchester, was elected an ordinary member of the Society.

Ordinary Meeting, November 12th, 1912.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

A vote of thanks was given to the donors of the books upon the table. These included : "*A Monograph of the Mycetozoa...*," by A. and G. Lister, 2nd ed. (8vo., London, 1911), and "*A Revision of the Ichneumonidae*," by Claude Morley (8vo., London, 1912), presented by the Trustees of the British Museum ; and "*Flora capensis*," vol. 5, sect. iii., pt. i., by Sir W. T. Thiselton-Dyer (8vo., London, 1912), purchased.

Professor S. J. HICKSON, F.R.S., exhibited a pearl alleged to have been found in a Nautilus. The pearl, which is irregularly pear-shaped, weighs 27·5 grains, and was presented to Professor Hickson by a Dutch magistrate in North Celebes.

A paper by Dr. HENRY WILDE, F.R.S., was read "**On Search-Lights and the 'Titanic' Disaster.**"

This paper is printed in full in the *Memoirs*.

Mr. H. G. J. MOSELEY, B.A., read a paper entitled "**Radium as a means of obtaining High Potentials.**" He stated that a radio-active substance which emits β-rays should, when insulated, continue to gain a positive charge until a potential of the order of a million volts is reached. Only the fastest β-rays should then be able to escape. Experiments have been made to test this point. A small bulb containing radium emanation was supported by a quartz rod in the centre of an exhausted flask. A disk suspended from a quartz spring in the neck of the flask formed a simple attracted disk electrometer. It was found that a bulb of diameter 9 m.m. reached a potential of 160,000 volts in the course of a few minutes. A sudden discharge then took place through the residual gas in the flask, although great care had been taken in obtaining the vacuum. A bulb of diameter 5 cm. charged up much more slowly: in the latter case no discharge took place, and the final potential, 140,000 volts, was limited by a leak of electricity along the quartz support. The cause of discharge in a high vacuum is still unknown.

A paper by Mr. C. G. DARWIN, B.A., entitled "**The Interference-phenomena produced by passing X-rays through crystals,**" was read by Mr. H G. J. MOSELEY.

General Meeting, November 26th, 1912.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

Mr. EDWARD MELLAND, *Kia Ora, Hale, Cheshire*, and Mr. J. E. MYERS, M.Sc., Beyer Fellow and Assistant Lecturer in Chemistry in the Victoria University of Manchester, *Acresfield, Gatley, Cheshire*, were elected ordinary members of the Society.

Ordinary Meeting, November 26th, 1912.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

A vote of thanks was accorded to the donors of the books upon the table. These included: "*Katalog und Ephemeriden veränderlicher Sterne für 1912*," by Ernst Hertwig (8vo., Leipzig, 1912), presented by the Remeis Sternwarte, Bamberg; "*The Crystallization of Metals*" (8vo., Glasgow, 1912), and "*Report on Diffusion in Solids*" (8vo., London, 1912), by Cecil H. Desch, presented by the Author; and *Festschrift des Vereins für Naturkunde zu Cassel zu Feier seines fünfundziebsigjährigen Bestehens*" (8vo., Cassel, 1912), presented by the Vereins für Naturkunde, Cassel.

A new exchange has been arranged with the Faraday Society (*Transactions*), London.

Dr. H. H. HOFFERT exhibited an instrument called 'The Rainbow Cup,' which was described by Mr. C. V. Boys, F.R.S., at a recent meeting of the Royal Society.

The PRESIDENT read a paper, entitled **"On the root-apex and young root of Lyginodendron."**

Dr. KURT LOEWENFELD read a paper on **"The importance of Autograph Documents in the History of Science" (Part II.).**

These papers will be printed in full in the *Memoirs*.

Ordinary Meeting, December 10th, 1912.

The President, Professor F. E. WEISS, D.Sc., F.L.S.,
in the Chair.

A vote of thanks was given to the donors of the books upon the table.

The PRESIDENT drew attention to the loss the Society had sustained by the death, on December 6th, of Mr. Arthur McDougall, B Sc. Mr. McDougall, who was elected an ordinary member on November 13th, 1866, had taken an active interest in the welfare of the Society, and from 1902 to 1910 he held the post of Treasurer.

Mr. T. A. COWARD, F.Z.S., exhibited a fossil believed to have been found in a brickfield near Timperley. The fossil, on examination by Professor Weiss, proved to be a barrel-shaped pith of a cycadean stem with small portions of the surrounding wood, the superficial markings of the fossil being due to the medullary rays.

A coloured photograph of a specimen of the Baikal or Formosan Teal, *Anas formosa*, was also shown by Mr. COWARD. The bird was shot at Wirral a short time ago. It had probably escaped from captivity, but there is always the possibility that these strong-winged birds may have wandered on migration. This Teal has occasionally reached Europe.

A paper entitled "**The Constitution of the Phosphoric Acids and some of their Alkali Salts**," by Dr ALFRED HOLT and Mr. J. E. MYERS, M.Sc., was read by the former.

There appear to be only two varieties of metaphosphoric acid and two corresponding series of salts. These salts are derived from mono and trimetaphosphoric acids.

The tri-acid is vitrious; the mono-acid can only be obtained in solution. The monometaphosphates of the alkalies are readily soluble in water and are prepared by either neutralizing the mono-acid or by devitrifying the glass obtained by the action of heat on microcosmic salt. This is in direct contradiction to the usual statement of text-books.

The more complex metaphosphates are probably double salts.

Miss PHILIPPA C. ESDAILE, M.Sc., read a paper on "**The Scientific Results of the Salmon Scale Research at Manchester University.**"

This paper is printed in full in the *Memoirs*.

Mr. C. L. BARNES, M.A., read a "**Note on the Mean Magnetic Moment and Mean Energy of a Vibrating Magnet,**" by Dr. J. R. ASHWORTH, communicated by Mr. R. L. TAYLOR, F.C.S., F.I.C.

This paper is printed in full in the *Memoirs*.

General Meeting, January 7th, 1913.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., F.C.S., Vice-President, in the Chair.

Mr. HANS RENOLD, M.I.Mech.E., *Priestnall Hey, Heaton Mersey*, was elected an ordinary member of the Society.

Ordinary Meeting, January 7th, 1913.

Mr. FRANCIS JONES, M.Sc., F.R.S.E., F.C.S., Vice-President, in the Chair.

A vote of thanks was accorded the donors of the books upon the table. Amongst these were: "*Rapporten van de Commissie in Nederlandsch-Indië voor Oudheidkundig Onderzoek op Java en Madoera,*" 1911 (4to., Batavia, &c., 1912), presented by the Bataviaasch Genootschap van Kunsten en Wetenschappen, Batavia; and parts of two new periodicals—*The Journal of the College of Agriculture*, presented by the Imperial University, Tokyo; and *The Science Reports, 2nd Series (Geology)*, presented by the Tôhoku Imperial University, Sendai.

A new exchange has been arranged with the Indian Association for the Cultivation of Science *(Bulletin)*, Calcutta.

Dr. G HICKLING made a short communication with regard to a remarkable band-like cloud, seen by him on the night of December 24th last, which it was suggested was possibly due to cloud formation on the trail of dust in the track of a meteorite.

Dr. H. F. COWARD described some experiments, carried out by Mr. F. BRINSLEY and himself, which led to the production of vortex rings of flame in mixtures of gases containing too little of the inflammable constituent for complete inflammation. The formation of vortex rings of flame in a hydrogen-air mixture was shown.

Papers, entitled "**The specification of the elements of stress,**" **Part II.**—"**Simplification of the specifications already given**" (vide *Manchester Memoirs*, Vol. LVI., No. 10), and **Part III.**—"**An Essay towards the reconstruction of the fundamental equations,**" were read by Mr. R. F. GWYTHER, M.A.

These papers will be printed in full in the *Memoirs.*

General Meeting, January 21st, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S.,
in the Chair.

Mr. CHARLES DUNCAN STEWART, B.Sc., *The Schools, Cheadle Hulme, Stockport,* was elected an ordinary member of the Society.

Ordinary Meeting, January 21st, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S.,
in the Chair.

A vote of thanks was given to the donors of the books upon the table. These included the following:—"*Katalog und Ephemeriden veränderlicher Sterne für 1913,*" by Ernst Hertwig (8vo., Leipzig, 1913), presented by the Remeis Sternwarte, Bamberg; "*Les Prix Nobel en 1911*" (8vo., Stockholm, 1912), presented by the Académie Royale Suédoise des Sciences, Stockholm; "*A Barometer Manual* ......" 7th ed. (8vo., London, 1912), and other publications presented by the Meteorological Office, London; and "*An Attempt to Establish the First Principles of Chemistry,*" vol. 1, by Thomas Thomson (8vo., London, 1825), presented by Mr. E. L. Rhead, M.Sc.Tech., F.I.C.

Dr. H. F. COWARD repeated the experiments shown at a previous meeting, carried out by himself and Mr. F. BRINSLEY, on the formation of vortex rings of flame in a mixture of air with about 6 per cent. of hydrogen.

A paper, entitled "**Experiments on Abel's theory that incombustible dusts act catalytically on igniting weak mixtures of methane and air,**" by Professor HAROLD B. DIXON, M.A., F.R.S. and HARRY MARCHANTON LOWE, B.Sc., was read by Professor Dixon.

The use of fine incombustible dusts as a means of preventing explosions of coal dust in mines has brought into prominence the conclusions arrived at by the late Sir Frederick Abel, viz., that the presence of such incombustible dusts in a mine may bring about the explosion of small percentages of fire-damp in air which would not otherwise be inflammable.

While the experiments on which Abel founded these conclusions have been repeated on a similar scale at the Home Office Experimental Station at Eskmeals during the past year with negative results, the explanation advanced by Abel has also been examined experimentally in the chemical laboratories of the Manchester University.

Abel's explanation is that the finely divided dust, heated up by the lamp flame, allows chemical action to take place on its surface—just as platinum brings about the combination of hydrogen and oxygen—and that the oxidation of the fire-damp proceeds with increased rapidity as the dust becomes more highly heated. The dust particles are thus raised to incandescence and fire the gas mixture round them.

This explanation involves the assumption that an amount of combustible gas, which is insufficient to propagate flame in the mixture, can by suffering partial combustion bring the remainder into an explosive state. The heating up of a gas mixture by an external source of heat increases its explosive power ; but this is not found to be true if the heat is derived from the burning of the gas itself.

The authors have heated up mixtures of coal gas and air and mixtures of methane and air by means of a long platinum spiral through which an electric current was passed. After chemical combustion is started the mixtures become *less* and not more explosive, although only a portion of the heat is derived from combustion of the gas itself. Even if the incombustible dusts acted like platinum it would be difficult to explain Abel's results as being due to a catalytic action.

Experiments carried out at Eskmeals show that the presence of fine incombustible dusts do not increase, but retard, the rate of explosion of gaseous mixtures.

Professor Dixon then proceeded to describe the Home Office Experimental Station at Eskmeals (Cumberland), in which the actual conditions in a mine are imitated as nearly as possible. This was illustrated by a number of lantern slides.

General Meeting, February 4th, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

Mr. J. B. HUBRECHT, M.A., Special Lecturer in the Victoria University of Manchester, of *Northfield, Knutsford, Cheshire*, was elected an ordinary member of the Society.

Ordinary Meeting, February 4th, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

A vote of thanks was accorded the donors of the books upon the table. Amongst these were :—"*The Centenary of a Nineteenth Century Geologist: Edward William Binney, F.R.S.*," by James Binney (12mo., Taunton, 1912), presented by Mr. C. L. Barnes, M.A. ; "*Narratives of Indian Captivity*" (8vo., Chicago, 1912), presented by the Newberry Library, Chicago ; "*The Lancashire Naturalist*," Vols. 4 and 5 (8vo., Darwen, 1911-1912), presented by Mr. T. A. Coward, F.Z.S.; and "*Manufacturing in Philadelphia, 1683-1912*," by T. J. Macfarlane (8vo., Philadelphia, 1912), presented by the Philadelphia Commercial Museum.

Mr. G. P. VARLEY, M.Sc., and Mr. ARTHUR ADAMSON, M.Sc.Tech., A.R.C.S., were nominated Auditors of the Society's accounts for the session 1912-13.

Mr. D. THODAY, M.A., **exhibited a capillary eudiometric apparatus** devised by Bonnier & Mangin, of Paris, for analysing small volumes of air in experiments on the exchange of gases between plants and the atmosphere. The apparatus was first described in 1891 by Antert. The volume of air used is only about 0.3 cc., and the complete analysis and subsequent washing of the apparatus take about a quarter of an hour. Mr. Thoday shewed how the apparatus is used and gave an account of his critical investigation of sources of error to which the method is liable and modifications of technique which he has been led to introduce. The results finally obtained shew a degree of accuracy which approaches very nearly to the highest expectation, a series of analyses giving percentages of CO_2 and oxygen in samples of air which differ from the mean by not more than 0.05 % of the total volume of air analysed.

Mr. W. B. BRIERLEY, M.Sc., read a paper entitled "**The Structure and Life-history of** *Leptosphaeria lemaneae*."

This paper is printed in full in the *Memoirs*.

General Meeting, February 18th, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

Mr. WILLIAM HENRY SUTCLIFFE, F.G.S., *Shore, Littleborough, Lancashire*, was elected an ordinary member of the Society.

Ordinary Meeting, February 18th, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

A vote of thanks was accorded to the donors of the books upon the table. These included the following :—"*Proceedings* *of the Meetings held .... the 100th Anniversary of the Founding of the Academy ....*" (Fol., Philadelphia, 1912), presented by the Academy of Natural Sciences, Philadelphia; "*Notice sur Henri Poincaré*," by Ernest Lebon (8vo., Paris, 1913), presented by the Author; and "*Guide to the Search Department of the Patent Office Library, with Appendices*," 4th ed. (12mo., London, 1913), presented by the Patent Office, London.

The PRESIDENT referred sympathetically to the disaster which had overtaken the Antarctic Expedition and the great loss sustained by the deaths of Captain R. F. Scott and his comrades. He drew attention to the necessity for the publication of a complete record of the scientific and geographical knowledge acquired by the *Terra Nova* Expedition.

Professor ELLIOT SMITH, M.A., M.D., F.R.S., gave an account of "**The Sussex Skull and its Brain-Cast.**" By the courtesy of Dr. Smith Woodward and Mr. Dawson, Professor Elliot Smith was able to exhibit plaster casts of the fragments of the Sussex Skull and the cast made from them to represent the formation of the brain. An account was given of the state of our knowledge of ancient man to illustrate the importance of the new information supplied by the Sussex remains.

Special Meeting, March 4th, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

At the Society's invitation, Mr. A. D. HALL, M.A., F.R.S., delivered a Special Lecture on "**The Plant and the Soil.**"

Ordinary Meeting, March 18th, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

A vote of thanks was accorded the donors of the books upon the table. Amongst these were:—"*Civil War Messages and Proclamations of Wisconsin War Governors*" (8vo., n.p., 1912), Wisconsin History Commission, Reprints No. 2, "*A Narrative of Service with the Third Wisconsin Infantry*," by J. W. Hinkley (8vo, n.p., 1912), presented by the Wisconsin History Commission, Madison; and "*Schnee- und Eisverhältnisse in Finland im Winter 1898-1899*," by W. W. Korhonen (fol. Kuopio, 1912), presented by the Helsingfors Meteorlogische Central-Anstalt.

Mr. THOMAS THORP, F.R.A.S., exhibited celloidine castings he had made of gratings ruled on Professor Rowland's machine at the Johns Hopkins University, U.S.A. These, having deeper grooves than those in ordinary use, gave much brighter spectra and were consequently of greater value.

A paper, entitled **"A Criticism of some Modern Tendencies in Prehistoric Anthropology,"** was read by Mr. W. H. SUTCLIFFE, F.G.S.

This paper is printed in full in the *Memoirs*.

Professor BOYD DAWKINS said he accepted the conclusions in Mr. Sutcliffe's timely and well-thought-out paper that follows the lines sketched out some thirty years ago in "Early Man in Britain," and brings the enquiry into the antiquity of man down to the knowledge of to-day. It emphasises the need of curbing the archæological imagination and of approaching the question by the strict inductive method by which all sciences have been evolved out of observations of varying merit, sound, non-proven, or false. The first of these three classes alone offers a safe basis for scientific theory, the second should be put, as Lord Avebury suggests, to a suspense account, and the third should go into the waste-paper basket. Applying these principles to the present aspect of the antiquity of man, we may take the following points to be clearly proved:—

1. That man is of vast and immeasurable antiquity, and that he was living in Europe as a hunter in the early Pleistocene age.

2. That the human skeletons found in the caves of Neanderthal, Spy, La Chapelle aux Saints, Quina, and Gibraltar, and in the Pleistocene river deposits of Mauer, near Heidelberg, and more recently of Piltdown, near Lewes, prove that the ancient inhabitants of those regions differed from existing types by the possession of simian characters.

3. That in the South of France men of modern types were represented in the middle and towards the close of the pleistocene age. The question of the pliocene age of man in Europe is non-proven and may be put to the suspense account. The evolution of the mammalia had arrived at the stage when living species of the eutherian mammalia had appeared, and therefore the time was ripe for the appearance of man. There are, however, no skeletons in evidence, and the chipped flints, "eoliths" and "eagle's beaks" and other forms cannot be taken to be proof of his handiwork, because they may be made without the intervention of man.

The Galley Hill skeleton, found in a pleistocene deposit near Northfleet, in Kent, some 25 years ago, may also go to the same suspense account, because the hotly-debated question as to their being the result of a later interment cannot ever be definitely settled. Both Sir John Evans and Professor Dawkins at the time believed that they were burials of later date than the pleistocene age. It is unfortunate that they should be taken by Dr. Keith and other competent craniologists in Britain and Germany to prove the presence of a modern type of man in Pleistocene Britain.

If, however, there be doubt as to the age of the Galley Hill interment, the evidence as to the presence of the same modern type at Ipswich, underneath undisturbed boulder clay, utterly breaks down. The skeleton lay in a hollow in the glacial sand, and was covered by surface soil common to the whole district, and here formed of decalcified re-arranged chalky boulder clays, proved by its presence on the top of an adjacent pit to be later than the clay. It belongs to a class of burials ranging from the neolithic to modern times.

The evidence as to the existence of man in Europe in the Eocene, Oligocene and Meiocene periods, based on the occurrence of "eoliths" is equally worthless, because it is now clearly proved that they may be, and in some cases have been, formed by natural causes, the movement and pressure of the gravels, etc. It also may be added to the anthropological refuse-heaps.

Mr. D. M. S. WATSON paid especial attention to the eoliths, which he refuses to recognise as artificial products.

Ordinary Meeting, April 8th, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S.,
in the Chair.

A vote of thanks was passed to the donors of the books upon the table.

MR. WILLIAM BURTON, M.A., F.C.S, read a "**Note on Black Pottery from Ashanti and the Gold Coast.**"

This paper is printed in full in the *Memoirs.*

A paper entitled "**The influence of Moisture in the air on Metabolism in the body**" was read by Mr. WILLIAM THOMSON, F.R.S.E., F.I.C.

This paper will be printed in full in the *Memoirs.*

Annual General Meeting, April 22nd, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

The Annual Report of the Council and the Statement of Accounts were presented, and it was resolved :—That the Annual Report, together with the Statement of Accounts, be adopted, and that they be printed in the Society's *Proceedings.*

Mr. F. H. CREWE and Mr. G. P. VARLEY were appointed Scrutineers of the balloting papers.

The following members were elected Officers of the Society and Members of the Council for the ensuing year :—

President: FRANCIS NICHOLSON, F.Z.S.

Vice-Presidents: F. E. WEISS, D.Sc., F.L.S.; FRANCIS JONES, M.Sc., F.R.S.E., F.C.S.; WILLIAM BURTON, M.A., F.C.S.; SYDNEY J. HICKSON, M.A., D.Sc., F.R.S.

Secretaries: R. L. TAYLOR, F.C.S., F.I.C.; GEORGE HICKLING, D.Sc.

Treasurer: W. HENRY TODD.

Librarian: C. L. BARNES, M.A.

Other Members of the Council: T. A. COWARD, F.Z.S.; G. ELLIOT SMITH, M.A., M.D., F.R.S.; W. W. HALDANE GEE, B.Sc., M.Sc.Tech., A.M.I.E.E.; BERTRAM PRENTICE, Ph.D., D.Sc.; R. F. GWYTHER, M.A.; H. R. HASSÉ, M.A., M.Sc.

Ordinary Meeting, April 22nd, 1913.

The President, Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

A vote of thanks was accorded to the donors of the books upon the table. Amongst these were :—"*Library of the Uni-*

versity of California: Contents-Index," vol. I. (8vo., Berkeley, Cal., 1889-1890), presented by the University of California; "*Annotated Catalogue of Newspaper Files in the Library of the State Historical Society of Wisconsin*," 2nd ed., by A. T. Griswold (8vo., Madison, Wiss., 1911), presented by the State Historical Society of Wisconsin; "*Report of the Committee appointed to report upon the Carboniferous Limestone Formation of the North of England, wi h special reference to its Coal Resources*," by Stanley Smith (8vo., Newcastle-upon-Tyne, 1912), presented by the North of England Institute of Mining and Mechanical Engineers; "*The Keuper Marls around Charnwood*," by T. O. Bosworth (8vo., Leicester, n.d.), presented by the Leicester Literary and Philosophical Society; and one hundred and sixty-two Dissertations, presented by the K. Christian-Albrechts Universitet, Kiel.

The PRESIDENT referred to the death, on April 11th, of Mr. ROBERT COTTON, M.Sc. Mr. Cotton had been a member of the Society since October 18th, 1910. The Society was represented at the funeral by Professor J. E. PETAVEL.

A question relating to the migration of birds was asked by Mr. C. L. BARNES, to which Mr. T. A. COWARD replied.

Mr. Francis Nicholson occupied the chair during the reading, by the PRESIDENT, of a paper entitled **"A Tylodendron-like Fossil."** Professor WEISS gave an account of the structure of a Tylodendron-like fossil recently exhibited to the Society by Mr. T. A. Coward.

Mr. WILFRID ROBINSON, B.Sc. (Lond.), read a paper entitled **"On some relations between** ***Puccinia malvacearum*** **(Mont.) and the tissues of its host plant** (*Althaea rosea*)."

These papers are printed in full in the *Memoirs*.

Ordinary Meeting, May 6th, 1913.

Professor F. E. WEISS, D.Sc., F.L.S., in the Chair.

A vote of thanks was accorded the donors of the books upon the table. The following were amongst the recent accessions to the Society's Library:—"*Flora Capensis*," vol. V., section III., pt. ii., by Sir William T. Thiselton-Dyer (8vo., London, 1913), purchased; and "*General Index to the Chemical News, Vols. 1 to 100*" (4to., London, 1913), purchased.

Mr ERNEST F. LANGE, M.I.Mech.E., A.M.Inst.C.E., F.C.S., read a paper entitled "**Bessemer, Göransson & Mushet.**"

A paper by Mr. H. S. HOLDEN, M.Sc., entitled "**On some abnormal specimens of** *Dictyota dichotoma*," was read by Professor F. E. WEISS, D.Sc., F.L.S.

These papers are printed in full in the *Memoirs*.

Extraordinary General Meeting, July 22nd, 1913.

The President, Mr. FRANCIS NICHOLSON, F.Z.S., in the Chair.

Dr. HENRY WILDE, F.R.S., read a paper, entitled "**On some new Multiple Relations of the Atomic Weights of Elementary Substances; and on the Classification and Transformations of Neon and Helium.**"

This paper is printed in full in the *Memoirs*.

MANCHESTER

LITERARY AND PHILOSOPHICAL SOCIETY.

Annual Report of the Council, April, 1913.

The Society had at the beginning of the session an ordinary membership of 154. Since then fifteen new members have joined the Society, ten members have resigned, and four members, Mr. Walter L. Behrens*, Mr. Alfred Brothers, F.R.A.S., Mr. Arthur McDougall, B.Sc., and Professor J. Dixon Mann, M.D., F.R.C.P., have died. Thus, at the end of the session, there are 155 ordinary members of the Society. The Society has further lost, by death, three honorary members, viz.: Mons. J. H. Poincaré, For.Mem. R.S., Professor Edward Strasburger, D.C.L., For.Mem. R.S., and Professor Ferdinand Zirkel, For.Mem. R.S. Memorial notices of these gentlemen will appear with this report in the *Memoirs and Proceedings.*

The average attendance at the meetings of the Society during the past session has been 32·4. This compares very favourably with the average of the previous session, 22·5; and with that of the session 1910-11, 21.

Twenty-eight papers have been read at the meetings during the year; 22 shorter communications have also been made.

* The obituary notice will appear in the next volume.

The Society commenced the session with a balance in hand, from all sources, of £400. 7s. 10d., made up as follows:—

At credit of General Fund	£46	14	7
,, ,, Wilde Endowment Fund...	253	14	7
,, ,, Joule Memorial Fund......	99	18	8
Balance 31st March, 1912..............	£400	7	10

At the close of the session the total balance in hand amounted to £263. 1s. 6d., the amounts standing at credit of the various accounts on the 31st March, 1913, being:—

At credit of General Fund....................	£142	2	7
,, ,, Wilde Endowment Fund...	116	11	5
,, ,, Joule Memorial Fund	4	7	6
Balance 31st March, 1913.......	£263	1	6

The Wilde Endowment Fund, kept as a separate banking account, shows a balance due to the Fund of £116. 11s. 5d. in its favour, as against a balance in hand of £253. 14s. 7d. at the end of the last financial year. The receipts for the year 1912-13 show a slight increase on the receipts for the year 1911-12.

The Librarian reports that during the session 876 volumes have been stamped, catalogued and pressmarked; 813 of these were serials, and 63 were separate works. 210 catalogue cards were written, 158 for serials, and 52 for separate works. The total number of volumes catalogued to date is 34,627, for which 12,189 cards have been written.

The library continues to be satisfactorily used for reference purposes. 238 volumes have been borrowed from the library during the past year. The number of books borrowed during the previous twelve months was 274, and during 1911-12, 185.

229 volumes have been bound during the year in 192 covers. The amount of binding for the previous session was 226 volumes bound in 189. In addition to this binding of periodicals, 64 works of permanent value have been repaired or rebound, a special grant having been made to meet this extra expenditure.

The additions to the library for the session amounted to 955 volumes, 828 serials, and 127 separate works. The donations (exclusive of the usual exchanges) were 126 volumes and 162 dissertations; 1 volume was purchased, in addition to those regularly subscribed for.

On the recommendation of a special Committee appointed to inquire what periodicals it was desirable to add to the series purchased by the Society,—consideration being given to the periodicals subscribed to by other libraries in Manchester,—two other journals, *The Annals of Botany* and *The New Phytologist*, have been added to the purchase list. The subscriptions to *The Journal of Botany*, *The Gardeners' Chronicle*, and the *Registrar-General's Annual Report* have been discontinued.

New exchanges have been arranged with the following:—Tôhoku Imperial University (*The Science Reports*, and *The Tôhoku Mathematical Journal*), Sendai; the Bureau of Productive Industries (*Icones Plantarum Formosanarum, nec non et Contributiones ad Floram Formosanam*), Formosa; Faraday Society (*Transactions*), London; Indian Association for the Cultivation of Science (*Bulletins*), Calcutta; and the International Institute of Agriculture (*Monthly Bulletins* and *Agricultural Statistics*), Rome.

The exchange with the Philosophical Society (*Bulletin*), Washington, has been discontinued.

Amongst the donations to the Society's Library during the year mention should be made of a gift by the President of volumes I—XX of *The Annals of Botany*. Gifts of books have also been made by Mr. George Barbour, Mr. C. L. Barnes, Mr. T. A. Coward, Mr. E. L. Rhead, Mr. A. W. Waters, and others. Mr. Waters presented to the library twenty-one volumes of the publications of the Meteorological Department of New South Wales.

The publication of the Society's *Memoirs and Proceedings* has been continued under the supervision of the Editorial Committee.

The sum of £249. 11s. 9d. has, at the request of Dr. Wilde, F.R.S., been transferred from the surplus income of the Wilde Endowment Fund to the Society's Funds.

Further accommodation for books and periodicals has been secured by the provision in the basement of a large bookcase, made from the design of Dr. Wilde. The Society is indebted to Dr. Wilde for the attention given by him to the carrying out of the work and for his interest in supervising other improvements and alterations.

The attention of the Council has been drawn to the fact that hitherto no special provision has been made for extinguishing fire in the Society's Rooms, and some simple forms of fire extinguishing apparatus have been provided, which, it is hoped, will serve the purpose sufficiently well.

A sum of £103. 6s. od., accumulated interest of the Joule Memorial Fund, has been invested in the East India Railway Company's 4 per cent. Annuity Stock.

Mr. A. D. Hall, F.R.S., at the Society's invitation, delivered a special lecture before the Society on March 4th. The lecture, which was entitled "The Plant and the Soil," was greatly appreciated by a large audience. It will be published in due course in the *Memoirs*. Mr. Hall was also the chief guest at the Annual Dinner which was held the same evening.

The Society has to lament the death of Mr. Arthur McDougall, B.Sc., who was a member for many years, and who served the Society for seven years as Honorary Treasurer.

The Committees appointed by the Council during the year were as follows:—

House and Finance.

The President.
Mr. Francis Jones.
Mr. C. L. Barnes.
Mr. R. L. Taylor.
Mr. F. Nicholson.
Mr. W. H. Todd.
Dr. H. G. A. Hickling.

Editorial.

The President.
Professor S. J. Hickson.
Dr. H. G. A. Hickling.
Professor E. Rutherford.
Mr. R. L. Taylor.
The Assistant Secretary.

Wilde Endowment.

The President.
Mr. Francis Jones.
Dr. H. G. A. Hickling.
Mr W. H. Todd.
Mr. R. L. Taylor.

Special Library Periodical Committee.

The President.
Professor S. J. Hickson.
Professor W. W. Haldane Gee.
Mr. R. L. Taylor.
Mr. C. L. Barnes.
Professor E. Rutherford.
Mr. R. F. Gwyther.
Dr. H. G. A. Hickling.
The Assistant Secretary.

Alfred Brothers, F.R.A.S., was born at Sheerness in 1826 and died at Handforth, Cheshire, on August 25th, 1912. He was the son of a chemist and druggist, but had the misfortune to lose his father when he was only 14 years of age, so that his educational opportunities were limited. For some time he served in a bookseller's shop at Maidstone, then he obtained employment in the survey of the Maidstone and Ashford Railway, and, later on, became an insurance collector, and thus came to Manchester, in 1855, as agent for an insurance company.

From the age of 16 he had been greatly attracted by the then novel subject of photography, which was afterwards to form the basis of his life's work, for when the insurance company failed he purc ased a photographic business in St. Ann's Square which had been started some few years before. At this time photography had made little progress beyond the stage of the Daguerreotype, which was followed successively by the old glass positive, and, finally, printing on paper from a negative.

Alfred Brothers soon became one of the recognized authorities on photography, and his patience and ingenuity entitle him to be considered as one of the pioneers in the development of that branch of science. His inteiest in photography was perhaps only equalled by his interest in astronomy, for he erected in his garden, first at Upper Brook Street and afterwards at Wilmslow, two fine telescopes made by Dancer, of Manchester. He is said to have taken the first successful photograph of the moon, and, in 1870, he was selected as the Government photographer to accompany the expedition sent to Syracuse to observe the eclipse of the sun, and he obtained the first complete photograph of the solar corona. He was also one of the pioneers in stellar photography.

Incidentally it is claimed for Mr. Brothers that he was the first to produce magnesium ribbon and to apply it as an adjunct in photography. In 1863 the manufacture of magnesium wire was commenced in Manchester, and he experimented with it, but found that the combustion was too slow to make it effective for photography. He one day tried passing a wire through the rollers of a burnishing machine and so made the first magnesium ribbon. One of the earliest applications he made with this was to photograph the caves of the Blue John Mine at Castleton, Derbyshire.

He was elected a member of this Society in 1860, and was for many years a constant attender at the meetings, serving as president at various times of the Photographic, Microscopical and Natural History section. He was a member of the Manchester Photographic Society almost from its commencement in 1855, and was one of those who founded the Astronomical Society of Manchester. His most important publication was a volume entitled "Photography: Its History, Processes, Apparatus and Materials," first edition, 1892; second edition, 1899, and he was engaged on a third edition at the time of his death.

W. B.

By the death on December 6th, 1912, of Mr. ARTHUR McDOUGALL, B.Sc., the Society has lost one of its oldest members and a very faithful friend. He was a son of the late Mr. Alexander McDougall, who came early in life from Scotland to settle in Manchester, and became a member of the Society in 1844.

Mr. McDougall studied chemistry at the Owens College under Professor Roscoe, graduated B.Sc. at the University of London, and then worked for some time in his father's laboratory on the products to be obtained from coal tar, with the result that many new compounds were introduced to commerce. Afterwards, along with his four brothers, he founded the City Flour Mills, with which he remained connected to the end of his life.

Mr. McDougall soon made his knowledge of chemistry available in connection with his daily work, and he investigated in particular the mineral constituents of wheat. But his energy found vent in other directions, and he occupied much time in working out improvements in the purification of coal gas, and invented the "McDougall furnace" designed for recovering the sulphur from the iron oxide employed to remove sulphuretted hydrogen from the gas. In 1898 Mr. McDougall turned his attention to the problem of preparing nitric acid synthetically.

His paper on this subject, written in collaboration with Mr. Frederick Howles, created great interest when read to the Society in March, 1900. It was entitled, "On the Production of Nitric Acid from air by means of the Electric Flame," and was undoubtedly the pioneer paper of a process which has since been worked successfully in many parts of the world. The paper is well worth perusal. The following extract will give some idea of the results obtained:—"By decreasing the high-tension current, so that only 0·15 ampere was used to feed the flame, 55 grms. of nitric acid were produced in an experiment extending over 9½ hours. This is equivalent to 300 grms. per horse-power per 12 hours, showing an increase over the quantities produced in all former experiments . . . and represents 51·5 per cent. of the amount theoretically obtainable from the amount of air passed in."

Mr. McDougall was very proud of his long connection with the Society. In a letter to the writer, dated January 10th, 1910, he says:—"My connection with the Society commenced when I was only 10 years of age. I had the run of Dr. Angus Smith's laboratory from about 1855 to 1859. About 1855 Dr. Smith began an investigation on the air of towns, and the Society's rooms being nearly in the centre of the town were used for the investigation, so that in a kind of way I have had a connection of a sort for over 50 years, and am familiar with all the recent career of the Society, of which I became a member as soon as ever I attained my majority. My father was also a member from 1844 and a fairly regular attender."

From 1903 to 1910 Mr. McDougall was Treasurer to the Society and was most careful and assiduous in carrying out the duties of his office till ill-health compelled his resignation.

Mr. McDougall's quiet manner and retiring disposition served to keep him somewhat in the background, but to those who knew him well his memory will long be cherished. His departure is greatly deplored by a large circle of friends.

F. J.

John Dixon Mann was the son of John Mann, at one time Borough Treasurer of Kendal. He was born at Kendal in 1840, and was 72 years of age at his death—on April 6th, 1912. He was educated at the Friends' School, Kendal, and served his apprenticeship to a medical man in that town. He then came to Manchester as a student of the Manchester Royal School of Medicine, took the M.R.C.S. and L.S.A. in 1862, and entered general practice in Manchester. In 1880 he took the degree of M.D. in the University of St. Andrews and the Membership of the Royal College of Physicians of London. In 1882 he was appointed Honorary Physician to the Salford Royal Hospital, a post which he still held at the time of his death.

In 1885, on the appointment of the late Dr. Cullingworth to the chair of Obstetrics, Dr. Dixon Mann succeeded him as Lecturer on Forensic Medicine and Toxicology in the Owens College, and was made Professor of the same subjects in 1892. During the last few years of his life he represented the University of Manchester on the General Medical Council.

His practice for the first twenty years was of a general character, but after that time became more restricted to the work of a consulting physician, and he was elected to the Fellowship of the Royal College of Physicians in 1890. During the latter part of his life he had a large consulting practice, and his opinion was highly valued by his colleagues as that of a physician of great practical experience, particularly well-balanced mind, and sound judgment.

In all his work he was essentially a scientific man, and he carried his scientific habit of mind into his medical work at a time when the practice of medicine was still largely empirical. He had a laboratory and workshop in his house, and was constantly occupied in experimental work, even when engaged in busy practice.

He became a member of the Manchester Literary and Philosophical Society in 1875, and his first published paper was in the *Proceedings* for 1878 (Vol. XVII., p. 91). It was "On an improved method of projecting Lissajous' figures on the Screen," and was his only communication to the Society. He published many papers in the *Lancet*, *British Medical Journal*, *Medical Chronicle* and other journals, and wrote two important books, viz.: "Forensic Medicine and Toxicology," published in 1893, which reached a 4th edition in 1908, and for which he was awarded the Swiney Prize in 1909. His other book on the "Physiology and pathology of the urine, with methods for its examination," was published in 1904, and embodied the results of a large amount of original experimental work.

In his earlier years he devoted much time to experiments bearing upon the applications of electricity to medical practice, but in later life he almost confined himself to chemical problems, more especially those bearing upon the action and excretion of poisons and the chemistry of the urine. He was for a time chairman of the Manchester and Salford Sanitary Association, President of the Manchester Medical Society, and of the Manchester Pathological Society, and examiner in the Universities of London, Oxford and Sheffield, as well as Manchester. He had been appointed Croonian Lecturer for 1914.

No notice, however brief, of Dr. Dixon Mann would be adequate without a reference to his love for music and his great ability as an organist. He frequently took the service at St. Peter's Church, and also was a composer for the organ, though he did not care for publication.

By his death Manchester loses a man of a type which can ill be spared—one who always sought the truth unflinchingly, who was content to labour continually for the sake of the work and not for the rewards the work might bring, and one of whom it might be difficult to say whether his modesty or his ability was the greater. R. B. W.

By the death of Jules Henri Poincaré the world of science has lost the most original and productive mathematical genius of his time. Primarily a pure mathematician, but interested rather in general principles and in the attainment of comprehensive points of view than in the details of calculation, he was led to scrutinize the methods of gravitational Astronomy and of Mathematical Physics in general, and was enabled to throw new light on questions which seemed to have attained the limits of their possible development. To Astronomy in particular he has given a new impetus by his theory of "Periodic Orbits," and the treatise on Celestial Mechanics, in which his researches are set forth, is held by competent judges to mark a step forward in the science as decisive almost as that made by Laplace. Not that his investigations have lightened in any degree, or simplified, the ordinary calculations of Astronomy, but they have pointed out the directions in which further light is to be sought on the evolution and the destinies of the planetary system. The catholicity of his sympathies and his grasp of general theories are shown also by the interest which he took in modern theories of electricity, which he has subjected to a profound and exhaustive examination. To men of science, outside the ranks of professed mathematicians, as well as to a wider circle, he is known by the collections of essays which he has published from time to time on questions of scientific philosophy, ranging from the foundations of mathematics to the latest cosmical and electrical speculations. Though dealing with almost all the profound questions of which he was a master, they are written in an easy and almost popular style, and illuminated by brilliant flashes of humour.

The external facts of his life are simple. He was born at Nancy on April 29th, 1854, of a distinguished Lorraine family; the actual President of the French Republic, for instance, M. Raymond Poincaré, is a near relative. He was educated partly at Nancy and partly at the Ecole Polytechnique and the Ecole des mines in Paris. He became Professor of Analysis at Caen, and subsequently occupied in succession various chairs of Mathematics in Paris. The various official or advisory posts which he held, and the academical and other distinctions which were conferred upon him, are far too numerous to be recounted here. The admiration felt for the magnitude and the continuity of his labours is heightened by the fact that his health was always weak and his constitution somewhat frail. His death, which was quite unexpected, took place on July 17th, 1912, at an age when the world had still great things to expect from him The *Dernières Pensées*, a posthumous collection of essays on his favourite topics, gives a pathetic indication that the evolution of his own thoughts was still in progress. H. L.

Professor EDUARD STRASBURGER, who died at Bonn on May 19th, 1912, was born at Warsaw in 1844. By his death Botany lost one who had played a large part in the construction of the science in its modern form. The dates of his numerous publications range from 1867 to 1911. He thus came into the science after the significance of the theory of evolution had been realised, and after the main comparisons of the various groups of the vegetable kingdom had been correctly established by earlier workers. The details of cell-structure were, however, only beginning to be known, and cytology, as we now understand it, was not even sketched out. Although Strasburger's work was by no means limited to this field, the cytological interest runs through most of it, and the pioneer part he took in the investigation of cell-structure and nuclear division would alone establish his great distinction as a botanist.

Strasburger's early work on the details of fertilisation in Bryophytes, Ferns, and Conifers led to his comprehensive investigations into the morphology of Gymnosperms, with special reference to the details of reproduction. To this period we owe "Die Coniferen und Gnetaceen" (1872) and "Die Angiospermen und die Gymnospermen" (1879). In 1875 his classic work "Über Zellbildung und Zelltheilung," which was subsequently published in enlarged second and third editions, appeared. From this period onwards his work was mainly cytological, and most advances in this field either originated or were critically modified by his work. The mere enumeration of the subjects dealt with is impossible here, but mention must be made of his paper on "The Periodic reduction of the number of chromosomes in the life-history of living organisms," read at the meeting of the British Association in 1894, and of his later investigations into the cytology of parthenogenesis, apogamy, and sex-distribution in dioecious plants. While these cytological studies were in progress, he published a large work on the conducting tissues of plants that is of fundamental importance both in plant-anatomy and in the physiology of the ascent of water in plants. Strasburger was a great investigator, and not only extended and deepened our knowledge of plants by his own work, but personally influenced many botanists, who worked in his research laboratory. His writing is clear, though it is by the matter rather than the method of exposition that he holds the attention. Similarly, his interests lay rather in the research laboratory than in more elementary teaching. He has, however, exerted great influence on botanical teaching in Europe and America through the "Botanische Praktikum," and later through the "Lehrbuch der Botanik," written by him and three other German botanists. Both these works have been translated into several languages, and in repeated editions have played an important part in modern botanical teaching. His popular work, "Rambles on the Riviera," the outcome of holiday studies, has also been translated into English.

Strasburger's scientific distinction was well recognised in this country. Besides being a Foreign Member of this Society he was a Foreign Member of the Royal Society and of the Linnæan Society, and received the medal of the latter society in 1905. W. H. L.

Geheimrath Prof. FERDINAND ZIRKEL was a native of Bonn, where he was born on May 20th, 1838. His early years were spent in that city, and he graduated in its University in 1861.

Fortunately, he was turned from his projected career as a mining engineer by the travels of his later student days, which led him, among other places, to Scotland, where so many others also have caught the passion for pure geology. At the same time he visited England, and doubtless learned then of the pioneer work of Sorby in the application of the microscope to the study of rocks. So early as 1862 he published an important investigation on microscopic petrography, and soon became the leading exponent of the new science. His "Lehrbuch" of 1866 was the first text-book of the subject. It was followed in 1873 by the more important "Mikroscopische Beschaffenheit der Mineralien und Gesteine," and soon after (1876) by the "Microscopical Petrography," published as one of the volumes of the "Exploration of the 40th Parallel."

To the scientific world at large, Zirkel was known as a great investigator and the author of very many papers on petrology and mineralogy. By very many he was greatly beloved as a teacher. He obtained his first professorship at the age of twenty-five, in the University of Lemberg. Five years later he went to Kiel, where he remained only two years. He was then offered the Professorship at Leipzig, which became his permanent scene of labour. The brilliance of his work and the affection of his pupils obtained him many honours. In this country he was made a Foreign Member of the Royal Society and of the Geological Society. He was elected an honorary member of this Society in April, 1888.

Zirkel maintained his activity and his love for petrology throughout a full life. In 1894 he completed a second edition of his "Lehrbuch," in reality a new work containing the fruit of thirty-five years' study, and taking its place at the head of the petrographical literature of the time. He continued his professional duties till close upon the time of his deeply lamented death, which took place on June 11th, 1912, robbing geology of one of its most notable figures on the European stage.

G. H.

MANCHESTER LITERARY A

Dr. *W. Henry Todd, Treasurer, in Account with*

	£ s. d.	£
To Balance, 1st April, 1912 ..		146 1
To Members' Subscriptions :—		
Half Subscriptions, 1910-11, 1 at £1. 1s. 0d.	1 1 0	
,, 1912-13, 20 ,, ,,	21 0 0	
Subscriptions :— 1910-11, 1 ,, £2. 2s. 0d.	2 2 0	
,, 1911-12, 8 ,, ,,	16 16 0	
,, 1912-13, 112 ,, ,,	235 4 0	
,, 1912-13, 1 ,, £2. 1s. 10d.	2 1 10	
		278
To Transfers from the Wilde Endowment Fund		233 1
To Sale of Publications		17
To Sale of Catalogues		0
To Dividends :—		
Natural History Fund	57 13 0	
Joule Memorial Fund	7 5 10	
		94 1
To Income Tax Refunded :—		
Natural History Fund	3 11 [illegible]	
Joule Memorial Fund	0 9 0	
Wilde Endowment Fund	21 0 0	
		25
To National Health Insurance Act deductions ..		1 1
To E. F. Lange, half-tone blocks ..		1
		£819 0

NATURAL HISTO

	£ s
To Balance, 1st April, 1912 ..	0 3
To Dividends on £1,225 Great Western Railway Company's Stock ..	57 13
To Remission of Income Tax, 1912 ..	3 11
To Balance against this Fund, 1st April, 1913 ..	4 13
	£66 6

JOULE MEMORI

	£ s.
To Balance, 1st April, 1912	99 18
To Dividends on £250 Loan to Manchester Corporation	7 5
To Remission of Income Tax, 1912	0 9
	£107 13

WILDE ENDOWMEN

	£ s.
To Balance, 1st April, 1912 ..	253 14
To Dividends on £7,500 Gas Light and Coke Company's Ordinary Stock ..	343 14
To Remission of Income Tax, 1912	21 0
To Bank Interest	2 8
	£620 17

HILOSOPHICAL SOCIETY.

iety, from 1st April, 1912, to 31st March, 1913. **Cr.**

	£ s. d.	£ s. d.
Charges on Property :—		
Chief Rent (Income Tax deducted)	12 3 4	
Income Tax	0 15 0	
Insurance against Fire	11 0 0	
		23 18 4
House Expenditure :—		
Coals, Gas, Electric Light, Water, &c.	39 9 3	
Tea, Coffee, &c., at Meetings	13 13 0	
Cleaning, Sweeping Chimneys, &c.	3 19 8½	
Replacements of mantles, crockery, dusters, ironware, etc.	6 18 11	
Fire Extinguishing Apparatus	2 17 6	
		6[illegible] 1[illegible] 9½
Administrative Charges :—		
Housekeeper	65 0 0	
Postages, and Carriage of Parcels and of "Memoirs"	34 6 7	
Stationery, Cheques, Receipts, and Engrossing	6 19 10	
Printing Circulars, Reports, &c.	10 13 6	
Extra attendance at Meetings, and during housekeeper's holidays	4 1 0	
Insurance against Liability	0 12 0	
National Health Insurance Stamps	3 1 6	
Miscellaneous Expenses	3 16 9½	
		123 12 1½
Publishing :—		
Printing "Catalogue of Serials"	76 2 0	
Printing "Memoirs and Proceedings"	126 12 0	
Illustrations for "Memoirs" (except Nat. Hist. papers)	2 0 0	
		[illegible]
Library :—		
Books and Periodicals (except those charged to Natural History Fund)	42 8 7	
Periodicals formerly subscribed for by the Microscopical and Natural History Section	4 7 11	
Binding and Repairing Books	0 17 [illegible]	
Catalogue Cards	0 15 0	
		57 3 6
Natural History Fund :— (Items shown in the Balance Sheet of this Fund below)		66 6 5
Joule Memorial Fund :— (Item shown in the Balance Sheet of this Fund below)		103 6 0
Wilde Endowment Fund (Income Tax refunded)		21 0 0
Balance at Williams Deacon's Bank, 1st April, 1913	136 10 1	
,, in Treasurer's hands	10 0 0	
		146 10 1
		£310 0 3

JND, 1912—1913. (Included in the General Account, above.)

	£ s. d.
Natural History Books and Periodicals	54 17 8
Illustrations for papers on Nat. Hist. in "Memoirs"	6 13 4
Binding Books	4 15 5
	£66 6 5

JND, 1912—1913. (Included in the General Account, above.)

	£ s. d.
Purchase of £100 East India Railway Company's 4 Annuity Stock	103 6 0
Balance, 1st April, 1913	4 7 6
	£107 13 6

JND, 1912—1913.

	£ s. d.
Assistant Secretary's Salary,	120 0 0
Maintenance of Society's Library :—	
Binding and Repairing Books	24 11 11
Book Case, &c.	49 11 0
Repairs and Improvements to Society's Premises	16 13 0
Legal Costs	6 12 4
Transfers to Society's Funds	283 12 0
Cheque Book	0 2 6
Balance at District Bank, 1st April, 1913	119 11 5
	£620 17 5

NOTE.—The Treasurer's Accounts of the Session 1912-1913 have been endorsed as follows:

April 10th, 1913. Audited and found correct.

We have also seen, at this date, the certificates of the following Stocks held in the name of the Society:—£1,225 Great Western Railway Company 5% Consolidated Preference Stock, Nos. 12,293, 12,294, and 12,323; £258 Twenty years' loan to the Manchester Corporation, redeemable 25th March, 1914 (No. 1,564); £7,500 Gas Light and Coke Company Ordinary Stock (No. 8/1960); and the deeds of the Natural History Fund, of the Wilde Endowment Fund, those conveying the land on which the Society's premises stand, and the Declarations of Trust.

Leases and Conveyances dated as follow:—

22nd Sept., 1797.
23rd Sept., 1797.
25th Dec., 1799.
" " "
22nd Dec., 1820.
23rd Dec., 1820.

Declarations of Trust:—

24th June, 1801.
23rd Dec., 1820.
8th Jan., 1878.

Appointment of New Trustees:—

30th April, 1851.

We have also verified the balances of the various accounts with the bankers' pass books.

(*Signed*) Geo. P. Varley.
A. Adamson.

THE COUNCIL
AND MEMBERS
OF THE
MANCHESTER
LITERARY AND PHILOSOPHICAL SOCIETY.

Corrected to November 20th, 1913.

President.
FRANCIS NICHOLSON, F.Z.S.

Vice-Presidents.
F. E. WEISS, D.Sc., F.L.S.
FRANCIS JONES, M.Sc., F.R.S.E., F.C.S.
WILLIAM BURTON, M.A., F.C.S.
SYDNEY J. HICKSON, M.A., D.Sc., F.R.S.

Secretaries.
R. L. TAYLOR, F.C.S., F.I.C.
GEORGE HICKLING, D.Sc., F.G.S.

Treasurer.
W. HENRY TODD.

Librarian.
C. L. BARNES, M.A.

Other Members of the Council.
T. A. COWARD, F.Z.S., F.E.S.
G. ELLIOT SMITH, M.A., M.D., F.R.S.
W. W. HALDANE GEE, B.Sc., M.Sc.Tech., A.M.I.E.E.
BERTRAM PRENTICE, Ph.D., D.Sc.
R. F. GWYTHER, M.A.
H. R. HASSÉ, M.A., M.Sc.

Assistant Secretary and Librarian.
R. F. HINSON.

ORDINARY MEMBERS.

Date of Election.

1911, April 4. Adamson, Arthur, M.Sc.Tech., A.R.C.S., Lecturer in Physics in the Municipal School of Technology, Manchester. *Municipal School of Technology, Sackville Street, Manchester.*

1901, Dec. 10. Adamson, Harold. *Oaklands Cottage, Godley, near Manchester.*

1912, Oct. 15. Adamson, R. Stephen, M.A., B.Sc., Lecturer in Botany in the Victoria University of Manchester. *The University, Manchester.*

1870, Dec. 13. Angell, John, F.C.S., F.I.C. 6, *Beacons-Field, Derby Road, Withington, Manchester.*

1865, Nov. 14. Bailey, Charles, M.Sc., F.L.S. *Haymesgarth, Cleeve Hill, S.O., Gloucestershire.*

1888, Feb. 7. Bailey, Sir William H., M.I.Mech.E., F.R.G.S. *Sale Hall, Cheshire.*

1895, Jan. 8. Barnes, Charles L., M.A. 151, *Plymouth Grove, Manchester.*

1903, Oct. 20. Barnes, Jonathan, F.G.S. *South Cliff House*, 301, *Great Clowes Street, Higher Broughton, Manchester.*

1910, Oct. 18. Beattie, Robert, D.Sc., Lecturer in Electrotechnics in the University of Manchester. *The University, Manchester.*

1895, Mar. 5. Behrens, Gustav. *Holly Royde, Withington, Manchester.*

1868, Dec. 15. Bickham, Spencer H., F.L.S. *Underdown, Ledbury.*

1875, Nov. 16. Boyd, John. *Barton House*, 11, *Didsbury Park, Didsbury, Manchester.*

1889, Oct. 15. Bradley, Nathaniel, F.C.S. *Sunnyside, Whalley Range, Manchester.*

1912, Oct. 15. Brierley, W.B., M.Sc., Lecturer in Economic Botany in the Victoria University of Manchester. *The University, Manchester.*

1889, April 16. Brooks, Samuel Herbert. *Slade House, Levenshulme, Manchester.*

1910, Nov. 1. Broome, James S., Science Teacher in the Salford Secondary School. 18, *Seedley Park Road, Pendleton, Manchester.*

1886, April 6. Brown, Alfred, M.A., M.D. *Sandycroft, Higher Broughton, Manchester.*

Date of Election

1889, Jan. 8. Brownell, Thomas William, F.R.A.S. 64, *Upper Brook Street, Manchester.*

1880, Oct. 15. Budenberg, C. F., M.Sc., M.I.Mech.E. *Bowdon Lane, Marple, Cheshire.*

1911, Jan. 10. Burt, Frank Playfair, B.Sc. (Lond.), D.Sc. (Bristol), Senior Lecturer in Chemistry in the Victoria University of Manchester. 15, *Oak Road, Withington, Manchester.*

1906, Feb. 27. Burton, Joseph, A.R.C.S. Dublin. *Tile Works, Clifton Junction, near Manchester.*

1894, Nov. 13 Burton, William, M.A., F.C.S. *The Hollies, Clifton Junction, near Manchester.*

1911, Oct. 31. Butterworth, Charles F. *Waterloo, Poynton, Manchester.*

1904, Oct. 18. Campion, George Goring, L.D.S. 264, *Oxford Street, Manchester.*

1907, Jan. 15. Carpenter, H. C. H., M.A., Ph.D., Professor of Metallurgy in the University of Manchester. 11, *Oak Road, Withington, Manchester.*

1890, Feb. 7. Chapman, D. L., M.A., F.R.S., Fellow and Tutor of Jesus College, Oxford. *Jesus College, Oxford.*

1901, Nov. 26. Chevalier, Reginald C., M.A., Mathematical Master at the Manchester Grammar School. 3, *Fort Road, Sedgley Park, Prestwich, Manchester.*

1907, Nov. 26. Clayton, Robert Henry, B.Sc., Chemist. 1, *Parkfield Road, Didsbury, Manchester.*

1895, April 30. Collett, Edward Pyemont. 8, *St. John Street, Manchester.*

1911, May 9. Cook, Gilbert, M.Sc., A.M.Inst.C.E., Vulcan Research Fellow in Engineering in the University of Manchester. 8, *Clarendon Road, Garston, Liverpool.*

1903, Oct. 20. Core, William Hamilton, M.Sc. *Groombridge House, Withington, Manchester.*

1906, Oct. 30. Coward, H. F., D.Sc., Chief Lecturer in Chemistry in the Municipal School of Technology, Manchester. *Municipal School of Technology, Sackville Street, Manchester,* and *216, Plymouth Grove, Manchester.*

1906, Nov. 27. Coward, Thomas Alfred, F.Z.S., F.E.S. *Brentwood, Bowdon, Cheshire.*

1908, Nov. 3. Cramp, William, M.Sc.Tech., M.I.E.E., Consulting Engineer. 20, *Mount Street, Manchester.*

1910, Oct. 4. Crewe, F. H., Assistant Science Master in the Municipal Secondary School, Whitworth Street. *Glengarth, Woodford Road, Bramhall.*

Date of Election.

1911, April 4. Darwin, C. G., B.A., Reader in Mathematical Physics in the University of Manchester. *The University, Manchester.*

1895, April 9. Dawkins, W. Boyd, M.A., D.Sc., F.R.S., Honorary Professor of Geology in the Victoria University of Manchester. *Fallowfield House, Fallowfield, Manchester.*

1894, Mar. 6. Delépine, A. Sheridan, M.B., B.Sc., Professor of Pathology in the Victoria University of Manchester. *Public Health Laboratory, York Place, Manchester.*

1887, Feb. 8. Dixon, Harold Baily, M.A., Ph.D., M.Sc., F.R.S., F.C.S., Professor of Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1906, Oct. 30. Edgar, E. C., D.Sc., Senior Lecturer in Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1910, Oct. 18. Evans, Evan Jenkin, B.Sc., Assistant Lecturer and Demonstrator in Physics in the University of Manchester. *The University, Manchester.*

1912. Oct. 15. Fairlie, D. M., M.Sc., Demonstrator in Electro-Chemistry in the Municipal School of Technology, Manchester. *The Municipal School of Technology, Sackville Street, Manchester.*

1912, Feb. 6. Forder, H. G., B.A., Senior Mathematical Master, High School, Cardiff. 33, *Wordsworth Avenue, Cardiff.*

1908, Jan. 28. Fox, Thomas William, M.Sc.Tech., Professor of Textiles in the School of Technology, Manchester University. 15, *Clarendon Crescent, Eccles.*

1912, Oct. 15. Garnett, J. C. Maxwell, M.A., Principal of the Municipal School of Technology, Manchester. *The Municipal School of Technology, Sackville Street, Manchester*, and *Westfield, Victoria Park, Manchester.*

1909, Mar. 23. Gee, W. W. Haldane, B.Sc., M.Sc.Tech., A.M.I.E.E., Professor of Pure and Applied Physics in the School of Technology, Manchester University. *Oak Lea, Whalley Avenue, Sale.*

1907, Oct. 15. Gravely, F. H., M.Sc. *Natural History Dept., Indian Museum, Calcutta.*

1907, Oct. 29. Gwyther, Reginald Felix, M.A., Secretary to the Joint Matriculation Board. 21, *Booth Avenue, Withington, Manchester.*

1911, Oct. 3. Hassé, H. R., M.A., M.Sc., Lecturer in Mathematics in the University of Manchester. 69, *Mauldeth Road, Withington, Manchester.*

Date of Election.

1902, Jan. 7. Hewitt, David B., M.D. 28, *Queen's Gardens, Hyde Park, London, W.*

1907, Oct. 15. Hickling, H. George A., D.Sc., F.G.S., Lecturer in Palæontology in the Victoria University of Manchester. *Glenside, Marple Bridge, near Stockport*

1895, Mar. 5. Hickson, Sydney J., M.A., D.Sc., F.R.S., Professor of Zoology in the Victoria University of Manchester. *The University, Manchester.*

1884, Jan. 8. Hodgkinson, Alexander, M.B., B.Sc. 18, *St. John Street, Manchester.*

1909, Jan. 12. Hoffert, Hermann Henry, D.Sc. (Lond.), A.R.S.M., His Majesty's Inspector of Schools. *Lime Grove, Brooklands, Sale.*

1909, Nov. 2. Holland, Sir Thomas H., K.C.I.E., D.Sc., F.R.S., Professor of Geology and Mineralogy in the University of Manchester, late Director of the Geological Survey of India. *Westwood, Alderley Edge, Cheshire.*

1905, Nov. 14. Holt, Alfred, M.A., D.Sc., Research Fellow of the University of Manchester. *Dowsefield, Allerton, Liverpool.*

1898, Nov. 29. Hopkinson, Sir Alfred, K.C., M.A., D.C.L., LL.D., late Vice-Chancellor of the Victoria University of Manchester. *Fairfield, Victoria Park, Manchester.*

1896, Nov. 3. Hopkinson, Edward, M.A., D.Sc., M.Inst.C.E. *Ferns, Alderley Edge, Cheshire.*

1909, Feb. 9. Howles, Frederick, M.Sc., Analytical and Research Chemist. *Glenluce, Waterpark Road, Broughton Park, Manchester.*

1889, Oct. 15. Hoyle, William Evans, M.A., D.Sc., F.R.S.E., Director of the Welsh National Museum, Cardiff. *City Hall, Cardiff.*

1913, Feb. 4. Hubrecht, J. B., M.A., Special Lecturer in the Victoria University of Manchester. *Northfield, Knutsford, Cheshire.*

1907, Oct. 15. Hübner, Julius, M.Sc.Tech., F.I.C., Lecturer in the Faculty of Technology in the University of Manchester. *Linden, Cheadle Hulme, Cheshire.*

1913, Oct. 21. Imms, A. D., M.A., D.Sc., Reader in Agricultural Entomology in the Victoria University of Manchester. *Department of Agricultural Entomology, The University, Manchester.*

Date of Election.

1899, Oct. 17. Ingleby, Joseph, M.I.Mech.E. *Springfield, Holly Road, Wilmslow, Cheshire*, and 20, *Mount Street, Manchester.*

1901, Nov. 26. Jackson, Frederick. 14, *Cross Street, Manchester.*

1870, Nov. 1. Johnson, William H., B.Sc. *Woodleigh, Altrincham.*

1911, Oct. 3. Johnstone, Mary A., B.Sc.(Lond.), Headmistress of the Municipal Secondary School for Girls, Whitworth Street, Manchester. 11, *Birchvale Drive, Romiley, near Manchester.*

1878, Nov. 26. Jones, Francis, M.Sc., F.R.S.E., F.C.S. *Manchester Grammar School*, and 17, *Whalley Road, Whalley Range, Manchester.*

1886, Jan. 12. Kay, Thomas, J.P. *Moorfield, Stockport, Cheshire.*

1903, Feb. 3. Knecht, Edmund, Ph.D., Professor of Chemistry in the School of Technology, Manchester University. *Beech Mount, Marple, Cheshire.*

1893, Nov. 14. Lamb, Horace, M.A., LL.D., D.Sc., Sc.D., F.R.S., Professor of Mathematics in the Victoria University of Manchester. 6, *Wilbraham Road, Fallowfield, Manchester.*

1909, Nov. 2. Lang, William H., M.B., C.M., D.Sc., F.R.S., F.L.S., Barker Professor of Cryptogamic Botany in the University of Manchester. 2, *Heaton Road, Withington, Manchester.*

1902, Jan. Lange, Ernest F., M.I.Mech.E., A.M.Inst.C.E., M.I. & S. Inst., F.C.S. *Fairholm*, 3, *Willow Bank, Fallowfield, Manchester.*

1911, Jan. 10. Lankshear, Frederick Russell, B.A. (New Zeal.), Demonstrator in Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1910, Oct. 18. Lapworth, Arthur, D.Sc., F.R.S., F.I.C., Professor of Organic Chemistry in the Victoria University of Manchester. 30, *Amherst Road, Withington, Manchester.*

1904, Mar. 15. Lea, Arnold W. W., M.D. 246, *Oxford Road, Manchester.*

1907, Oct. 29. Leigh, Harold Shawcross. *Brentwood, Worsley.*

1908, Oct. 20. Liebert, Martin, Ph.D., Managing Director of Meister Lucius and Brüning, Ltd., Manchester. 1, *Lancaster Road, Didsbury, Manchester.*

912, Nov. 12. Lindsey, Miss Marjorie, B.Sc., Research Student in the Victoria University of Manchester. 3, *Demesne Road, Whalley Range, Manchester.*

Date of Election.

1912, May 7. Loewenfeld, Kurt, Ph.D. *Fern Bank, Ogden Road, Bramhall, Cheshire.*

1857, Jan. 27. Longridge, Robert Bewick, M.I.Mech.E. *Yew Tree House, Tatley, Knutsford, Cheshire.*

1910. Oct. 18. McDougall, Robert, B.Sc. *City Flour Mills, German Street, Manchester.*

1912. Oct. 15. McFarlane, John, M.A. (Edin.), B.A. (Cantab), M.Com. (Manc.), Lecturer in Geography in the Victoria University of Manchester. *The University, Manchester.*

1905, Oct. 31. McNicol, Mary, M.Sc. 182, *Upper Chorlton Road, Manchester.*

1904, Nov. 1. Makower, Walter, B.A., D.Sc. (Lond.), Lecturer in Physics in the University of Manchester. *Mayanas, Brook Road, Fallowfield, Manchester.*

1902, Mar. 4. Mandleberg, Goodman Charles. *Redclyffe, Victoria Park, Manchester.*

1911, Oct. 31. March, Margaret Colley, M.Sc. *The University, Edinburgh.*

1901, Dec. 10. Massey, Herbert. *Ivy Lea, Burnage, Didsbury, Manchester.*

1894, Nov. 1. Mather, Sir William, P.C., M.Inst.C.E., M.I.Mech.E. *Iron Works, Salford.*

1912, Nov. 26. Melland, Edward. *Kia Ora, Hale, Cheshire.*

1873, Mar. 18. Melvill, James Cosmo, M.A., D.Sc., F.L.S. *Meole Brace Hall, Shrewsbury.*

1894, Feb. 6. Mond, Robert Ludwig, M.A., F.R.S.E., F.C.S. *Winnington Hall, Northwich, Cheshire.*

1911, May 9. Moseley, Henry Gwyn Jeffreys, B.A., Lecturer in Physics in the University of Manchester. *Physical Laboratories, The University, Manchester.*

1911, Oct. 3. Mumford, A. A., M.D. Medical Officer, Manchester Grammar School. 44, *Wilmslow Road, Withington, Manchester*, and *25, St. Ann's Street, Manchester.*

1912, Nov. 26. Myers, J. E., M.Sc., Beyer Fellow and Assistant Lecturer in Chemistry in the Victoria University of Manchester. *Acresfield, Gatley, Cheshire.*

1908, Jan. 28. Myers, William, Lecturer in Textiles in the School of Technology, Manchester University. *Acresfield, Gatley, Cheshire.*

1873, Mar. 4. Nicholson, Francis, F.Z.S. *The Knoll, Windermere, Westmorland.*

Date of Election.

1884, April 15. Okell, Samuel, F.R.A.S. *Overley, Langham Road, Bowdon, Cheshire.*

1892, Nov. 15. Perkin, W. H., Sc.D., Ph.D., M.Sc., F.R.S., Waynflete Professor of Chemistry in the University of Oxford. *The University, Oxford.*

1901, Oct. 29. Petavel, J. E., B.A., D.Sc., F.R.S., Professor of Engineering in the Victoria University of Manchester. *The University, Manchester.*

1885, Nov. 17. Phillips, Henry Harcourt, F.C.S. *Lynwood, Turton, near Bolton, Lancs.*

1903, Dec. 15. Prentice, Bertram, Ph.D., D.Sc., Principal, Royal Technical Institute, Salford. *Isca Mount, Manchester Road, Swinton.*

1911, Oct. 17. Pring, J. N., D.Sc., Lecturer and Demonstrator in Electro-Chemistry in the University of Manchester. *The University, Manchester.*

1901, Dec. 10. Ramsden, Herbert, M.D. (Lond.), M.B., Ch.B. (Vict.) *Sunnyside, Dobcross, near Oldham, Lancs.*

1888, Feb. 21. Rée, Alfred, Ph.D., F.C.S. 15, *Mauldeth Road, Withington, Manchester.*

1913, Jan 7. Renold, Hans, M.I.Mech.E. *Priestnall Hey, Heaton Mersey, near Manchester.*

1910, Oct. 4. Rhead, E. L., M.Sc.Tech., F.I.C., Lecturer on Metallurgy at the Municipal School of Technology, Manchester. *Stonycroft, Polygon Avenue, Levenshulme, Manchester.*

1912, Oct. 29. Roberts, A. W. Rymer, M.A., *Ellerbeck, Crook, near Kendal.*

1880, Mar. 23. Roberts, D. Lloyd, M.D., F.R.S.E., F.R.C.P. (Lond.) *Ravenswood, Broughton Park, Manchester.*

1911, Jan. 10. Robinson, Robert, D.Sc. (Vict.), Teacher of Chemistry in the Victoria University of Manchester. *Field House Chesterfield.*

1910, Oct. 18. Rossi, Roberto, M.Sc. *Trinity College, Cambridge.*

1897, Oct. 19. Rothwell, William Thomas. *Heath Brewery, Newton Heath, near Manchester.*

1907, Oct. 15. Rutherford, Ernest, M.A., D.Sc., F.R.S., Langworthy Professor of Physics in the University of Manchester. 17, *Wilmslow Road, Withington, Manchester.*

Date of Election.

1911, Oct. 17. Sandiford, Peter, M.Sc. (Manc.), Ph.D. (Columbia). *Faculty of Education, The University, Toronto, Canada.*

1909, Jan. 26 Schmitz, Hermann Emil, M.A., B.Sc., Physics Master at the Manchester Grammar School. 15, *Brighton Grove, Rusholme, Manchester.*

1873, Nov. 18. Schuster, Arthur, Sc.D., Ph.D., Sec.R.S., F.R.A.S., Honorary Professor of Physics in the Victoria University of Manchester. *Yeldall, Twyford, Berks.*

1898, Jan. 25. Schwabe, Louis. *Hart Hill, Eccles Old Road, Pendleton, Manchester.*

1890, Nov. 4. Sidebotham, Edward John, M.A., M.B., M R.C.S. *Erlesdene, Bowdon, Cheshire.*

1903, April 28. Sidebottom, Henry. *Woodstock, Bramhall, Cheshire.*

1910, Oct. 4. Smith, Grafton Elliot, M.A., M.D., F.R.S., Professor of Anatomy in the University of Manchester. *The University, Manchester.*

1906, Nov. 27. Smith, Norman, D.Sc., Assistant Lecturer in Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1896, Feb. 18. Spence, David. *Lowood, Hindhead, Haslemere, R.S.O., Surrey.*

1901, Dec. 10. Spence, Howard. *Audley, Broad Road, Sale, Cheshire.*

1911, Oct. 17. Start, Laura, Lecturer in Art and Handicraft in the University of Manchester. *Moor View, Mayfield Road, Kersal, Manchester.*

1913, Jan. 21. Stewart, Charles Duncan, B.Sc. *Kew Observatory, Richmond, Surrey.*

1897, Nov. 30. Stromeyer, C. E., M.Inst.C.E *Steam Users' Association,* 9, *Mount Street, Albert Square, Manchester,* and *Landfield, West Didsbury.*

1910, Oct. 18. Tattersall, Walter Medley, D.Sc., Keeper of the Manchester Museum. *The Museum, University, Manchester.*

1895, April 9. Tatton, Reginald A., M.Inst.C.E., Engineer to the Mersey and Irwell Joint Committee. *Manor House, Chelford, Cheshire.*

1893, Nov. 14. Taylor, R. L., F.C.S., F.I.C. *Municipal Secondary School, Whitworth Street,* and 4, *St. Werburgh's Road, Chorlton-c.-Hardy, Manchester.*

Date of Election.

1906, April 10. Thewlis, Councillor J. H. *Daisy Mount, Victoria Park, Manchester.*

1911, Oct. 17. Thoday, D., M.A., Lecturer in Plant Physiology in the University of Manchester. *The University, Manchester.*

1911. Jan. 10. Thomson, J. Stuart, Ph.D. (Bern), Senior Demonstrator in Zoology in the Victoria University of Manchester. *The University, Manchester.*

1873, April 15. Thomson, William, F.R.S.E., F.I.C., F.C.S. *Royal Institution, Manchester.*

1896, Jan. 21. Thorburn, William, M.D., B.Sc. 2, *St. Peter's Square, Manchester.*

1896, Jan. 21. Thorp, Thomas, F.R.A.S. *Moss Bank, Whitefield, near Manchester.*

1899, Oct. 17. Todd, William Henry. *Rivington, Irlam Road, Flixton, near Manchester.*

1909, Jan. 26. Varley, George Percy, M.Sc. (Vic.), Assistant Master in the Municipal Secondary School, Manchester. 19, *Mayfield Road, Whalley Range, Manchester.*

1912. Oct. 15. Walker, Miles, M.A , M.I.E.E., Professor of Electrical Engineering, the Municipal School of Technology, Manchester. *The Cottage, Leicester Road, Hale, Altrincham.*

1873, Nov. 18. Waters, Arthur William, F.L.S., F.G.S. *Alderley, McKinley Road, Bournemouth.*

1906, Nov. 13. Watson, D. M. S., M.Sc. 60, *Lissenden Mansions, Highgate Road, London, N.W.*

1892, Nov. 15. Weiss, F. Ernest, D.Sc., F.L.S., Acting Vice-Chancellor and Professor of Botany in the Victoria University of Manchester. *Easedale, Disley, Cheshire.*

1909, Feb. 9. Weizmann, Charles, Ph.D., D.Sc., Reader in Bio-Chemistry in the Victoria University of Manchester. *The University, Manchester.*

1908, May 12. Welldon, Rt. Rev. J. E. C., D.D., Dean of Manchester *The Deanery, Manchester.*

Date of Election.

1911, Oct. 17. West, Tom, B.Sc., Chemist and Metallurgist. *101, Spring Bank Street, Stalybridge, near Manchester.*

1901, Oct. 1. Wild, Robert B., M.D., M.Sc., F.R.C.P., Professor of Materia Medica and Therapeutics in the Victoria University of Manchester. *Broome House, Fallowfield, Manchester.*

1859, Jan. 25. Wilde, Henry, D.Sc., D.C.L., F.R.S. *The Hurst, Alderley Edge, Cheshire.*

1909, Jan. 26. Wolfenden, John Henry, B.Sc. (Lond.), A.R.C.S. (Lond.), Assistant Master in the Municipal Secondary School, Manchester. 13, *Pole Lane, Failsworth.*

1905, Oct. 31. Woodall, Herbert J., A.R.C.S. 32, *Market Place, Stockport.*

1860, April 17. Woolley, George Stephen. *Victoria Bridge, Manchester.*

1863, Nov. 17. Worthington, Samuel Barton, M.Inst.C.E., M.I.Mech.E. *Mill Bank, Bowdon,* and 37, *Princess Street, Manchester.*

1895, Jan. 8. Worthington, Wm. Barton, B.Sc., M.Inst.C.E. *Kirkstyles, Duffield, near Derby.*

N.B.—Of the above list the following have compounded for their subscriptions, and are therefore life members:—

Bailey, Charles, M.Sc., F.L.S.

Bradley, Nathaniel, F.C.S.

Ingleby, Joseph, M.I.Mech.E.

Johnson, William H., B.Sc.

Worthington, Wm. Barton, B.Sc., M.Inst.C.E.

HONORARY MEMBERS.

Date of Election.

1892, April 26. Abney, Sir William de W., K.C.B., D.C.L., D.Sc., F.R.S. *Rathmore Lodge, Bolton Gardens South, South Kensington, London, S.W.*

1892, April 26. Amagat, É. H., For. Mem. R.S., Memb. Inst. Fr. (Acad. Sci.), Examinateur à l'École Polytechnique. *Avenue d'Orléans*, 19, *Paris.*

1894, April 17. Appell, Paul, Membre de l'Institut, Professor of Theoretical Mechanics. *Faculté des Sciences, Paris.*

1889, April 30. Avebury, Right. Hon. John Lubbock, Lord, D.C.L., LL.D., F.R.S. *High Elms, Down, Kent.*

1892, April 26. Baeyer, Adolf von, For. Mem. R.S., Professor of Chemistry in the University of Munich. 1, *Arcisstrasse, Munich.*

1886, Feb. 9. Baker, John Gilbert, F.R.S., F.L.S. 3, *Cumberland Road, Kew.*

1889, April 30. Carruthers, William, F.R.S., F.L.S. 44, *Central Hill, Norwood, London, S.E.*

1903, April 28. Clarke, Frank Wigglesworth, D.Sc. *United States Geological Survey, Washington, D.C., U.S.A.*

1866, Oct. 30. Clifton, Robert Bellamy, M.A., F.R.S., F.R.A.S., Professor of Experimental Philosophy in the University of Oxford. 3, *Bardwell Road, Banbury Road, Oxford.*

1892, April 26. Curtius, Theodor, Professor of Chemistry in the University of Kiel. *Universität, Kiel.*

1892, April 26. Darboux, J. Gaston, Membre de l'Institut, Secrétaire perpétuel de l'Académie des Sciences, Doyen honoraire de la Faculté des Sciences. 3, *Rue Mazarine, Paris.*

Date of Election.

1894, April 17. Debus, H., Ph.D., F.R.S. 4. *Schlangenweg, Cassel, Hessen, Germany.*

1900, April 24. Dewar, Sir James, M.A., LL.D., D.Sc., F.R.S., V.P.C.S., Fullerian Professor of Chemistry at the Royal Institution. *Royal Institution. Albemarle Street, London, W.*

1892, April 26. Edison, Thomas Alva. *Orange, N.J., U.S.A.*

1895, April 30. Elster, Julius, Ph.D. 6, *Lessingstrasse, Wolfenbuttel.*

1900, April 24. Ewing, Sir J. Alfred, K.C.B., M.A., LL.D., F.R.S., Director of Naval Education to the Admiralty. *Frog-hole, Edenbridge, Kent.*

1889, April 30. Farlow, W. G., Professor of Botany at Harvard College. *Harvard College, Cambridge, Mass., U.S.A.*

1900, April 24. Forsyth, Andrew Russell, M.A., Sc.D., LL.D., F.R.S. Professor of Mathematics at the Imperial College of Science and Technology. *The Imperial College of Science and Technology, S. Kensington, London.*

1892, April 26. Fürbringer, Max, Professor of Anatomy in the University of Heidelberg. *Universität, Heidelberg.*

1900, April 24. Geikie, James, D.C.L., LL.D., F.R.S., Murchison Professor of Geology and Mineralogy in the University of Edinburgh. *Kilmorie, Colinton Road, Edinburgh.*

1895, April 30. Geitel, Hans. 6, *Lessingstrasse, Wolfenbuttel.*

1894, April 17. Glaisher, J. W. L., Sc.D., F.R.S. *Trinity College, Cambridge.*

1894, April 17. Gouy, A., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Physics in the University of Lyons. *Faculté des Sciences, Lyons.*

1900, April 24. Haeckel, Ernst, Ph.D., Professor of Zoology in the University of Jena. *Zoologisches Institut, Jena.*

1894, April 17. Harcourt, A. G. Vernon, M.A., D.C.L., F.R.S., V.P.C.S. *St. Clare, Ryde, Isle of Wight.*

1894, April 17. Heaviside, Oliver, Ph.D., F.R.S. *Homefield, Lower Warberry, Torquay.*

1892, April 26. Hill, G. W. *West Nyack, N.Y., U.S.A.*

Date of Election.

1888, April 17. Hittorf, Johann Wilhelm, Professor of Physics at Münster, *Polytechnicum, Münster.*

1892, April 26. Klein, Felix, Ph.D., For. Mem. R.S., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Mathematics in the University of Göttingen. 3, *Wilhelm Weber Strasse, Göttingen.*

1894, April 17. Königsberger, Leo, Professor of Mathematics in the University of Heidelberg. *Universität, Heidelberg.*

1902, May 13. Larmor, Sir Joseph, M.A., D.Sc., LL.D., F.R.S., F.R.A.S. *St. John's College, Cambridge.*

1892, April 26. Liebermann, C., Professor of Chemistry in the University of Berlin. 29, *Matthäi-Kirch Strasse, Berlin.*

1887, April 19. Lockyer, Sir J. Norman, K.C.B., LL.D., Sc.D., F.R.S., Corr. Memb. Inst. Fr. (Acad. Sci.). *Hill Observatory, Salcombe Regis, Sidmouth, Devon.*

1902, May 13. Lodge, Sir Oliver Joseph, D.Sc., LL.D., F.R.S., Principal of the University of Birmingham. *The University, Birmingham.*

1900, April 24. Lorentz, Hendrik Anton, For. Mem. R.S., Corr. Memb. Inst. Fr. (Acad. Sci.), Professor of Physics in the University of Haarlem *Zijlweg, 76, Haarlem.*

1892, April 26. Marshall, Alfred, M.A., formerly Professor of Political Economy in the University of Cambridge. *Balliol Croft, Madingley Road, Cambridge.*

1901, April 23. Metschnikoff, Élie, D.Sc., For. Mem. R.S. *Institut Pasteur, Paris.*

1895, April 30 Mittag-Leffler, Gösta, D.C.L. (Oxon.), For. Mem. R.S., Professor of Mathematics in the University of Stockholm, *Djursholm, Stockholm.*

1894, April 17. Murray, Sir John, K.C.B., LL.D., Sc.D., Ph.D., F.R.S. F.L.S. *Challenger Lodge, Wardie, Edinburgh.*

1910, April 5. Nernst, Geh. Prof. Dr. Walter, Director of the Physikal-Chemisches Institut in the University of Berlin. *Am Karlsbad 26a Berlin W. 35.*

Date of Election

1902, May 13. Osborn, Henry Fairfield, Professor of Vertebrate Palæontology at Columbia College. *American Museum of Natural History, W. 77 Street, New York, U.S.A.*

1894, April 17. Ostwald, W., Professor of Chemistry. *Grosbothen, Kgr. Sachsen.*

1899, April 25. Palgrave, Sir Robert H. Inglis, F.R.S., F.S.S. *Henstead Hall, Wrentham, Suffolk.*

1894, April 17. Pfeffer, Wilhelm, For. Mem. R.S., Professor of Botany in the University of Leipsic. *Botanisches Institut, Leipsic.*

1892, April 26. Quincke, G. H., For. Mem. R.S., Professor of Physics in the University of Heidelberg. *Universität, Heidelberg.*

1899, April 25. Ramsay, Sir William, K.C.B., Ph.D.,. Sc.D., M.D., F.R.S., Professor of Chemistry in University College, London. 19, *Chester Terrace, Regent's Park, London, N.W.*

1886, Feb. 9. Rayleigh, Right Hon. John William Strutt, Lord, O.M., M.A., D.C.L. (Oxon.), Sc.D. (Cantab.), LL.D. (Univ. McGill), F.R.S., F.R.A.S., Corr. Memb. Inst. Fr. (Acad. Sci.), Chancellor of the University of Cambridge. *Terling Place, Witham, Essex.*

1900, April 24. Ridgway, Robert, Curator of the Department of Birds, U.S. National Museum. *Brookland, District of Columbia, U.S.A.*

1897, April 27. Roscoe, Right Hon. Sir Henry Enfield, B.A., D.C.L. LL.D., F.R.S., V.P.C.S., Corr. Memb. Inst. Fr. (Acad. Sci.). 10, *Bramham Gardens, Earl's Court, London, S.W.*

1902, May 13. Scott, Dukinfield Henry, M.A., LL.D.. Ph.D., F.R.S., F.L.S. *East Oakley House, Oakley, Hants.*

1892, April 26. Solms, H., Graf zu, Professor of Botany in the University of Strassburg. *Universität, Strassburg.*

1895, April 30. Suess, Eduard, Ph.D., For. Mem. R.S., For. Assoc. Inst. Fr. (Acad. Sci.), Professor of Geology in the University of Vienna. 9, *Africanergasse, Vienna.*

Date of Election.

1892, April 26 Thiselton-Dyer, Sir William T., K.C.M.G., C.I.E., M.A., Sc.D., Ph.D., LL.D., F.R.S. Lately Director Royal Botanic Gardens, Kew. *The Ferns, Witcombe, Gloucester.*

1895, April 30. Thomson, Sir Joseph John, O.M., M.A., Sc.D., F.R.S., Cavendish Professor of Experimental Physics in the University of Cambridge. *Trinity College, Cambridge.*

1894, April 17. Thorpe, Sir T. Edward, C.B., Ph.D., D.Sc., LL.D., F.R.S., V.P.C.S. *Whinfield, Salcombe, S. Devon.*

1894. April 17. Turner, Sir William, K.C.B., M.B., D.C.L., LL.D., Sc.D., F.R.S., F.R.S.E., Professor of Anatomy in the University of Edinburgh. 6, *Eton Terrace, Edinburgh.*

1886, Feb. 9 Tylor, Sir Edward Burnett, D.C.L. (Oxon), LL.D. (St. And. and McGill Univs.), F.R.S., formerly Professor of Anthropology in the University of Oxford. *Linden, Wellington, Somerset.*

1894, April 17. Vines, Sidney Howard, M.A., D.Sc., F.R.S., F.L.S., Sherardian Professor of Botany in the University of Oxford. *Headington Hill, Oxford.*

1891, April 17. Warburg, Emil, Professor of Physics at the Physical Institute, Berlin. *Physikalisches Institut, Neue Wilhelmstrasse, Berlin.*

1894, April 17. Weismann, August, For. Mem. R.S., Professor of Zoology in the University of Freiburg. *Universität, Freiburg i. Br.*

CHANGES OF ADDRESS.

Members are particularly requested to inform the Secretaries of any errors in their addresses or descriptions.

Awards of the Dalton Medal.

1898. Edward Schunck, Ph.D., F.R.S.

1900. Sir Henry E. Roscoe. F.R.S.

1903. Prof. Osborne Reynolds, LL.D., F.R.S.

THE WILDE LECTURES.

1897. (July 2.) "On the Nature of the Röntgen Rays." By Sir G. G. STOKES, Bart., F.R.S. *(28 pp.)*

1898. (Mar. 29.) "On the Physical Basis of Psychical Events." By Sir MICHAEL FOSTER, K.C.B., F.R.S. *(46 pp.)*

1899. (Mar. 28.) "The newly discovered Elements; and their relation to the Kinetic Theory of Gases." By Prof. WILLIAM RAMSAY, F.R.S. *(19 pp.)*

1900. (Feb. 13.) "The Mechanical Principles of Flight." By the Rt. Hon. LORD RAYLEIGH, F.R.S. *(26 pp.)*

1901. (April 22.) "Sur la Flore du Corps Humain." By Dr. ÉLIE METSCHNIKOFF, For.Mem.R.S. *(38 pp.)*

1902. (Feb. 25.) "On the Evolution of the Mental Faculties in relation to some Fundamental Principles of Motion." By Dr. HENRY WILDE, F.R.S. *(34 pp., 3 pls.)*

1903. (May 19.) "The Atomic Theory." By Professor F. W. CLARKE, D.Sc. *(32 pp.)*

1904. (Feb. 23.) "The Evolution of Matter as revealed by the Radio-active Elements." By FREDERICK SODDY, M.A. *(42 pp.)*

1905. (Feb. 28.) "The Early History of Seed-bearing Plants, as recorded in the Carboniferous Flora." By Dr. D. H. SCOTT, F.R.S. *(32 pp., 3 pls.)*

1906. (March 20.) "Total Solar Eclipses." By Professor H. H. TURNER, D.Sc., F.R.S. *(32 pp.)*

1907. (February 18.) "The Structure of Metals." By Dr. J. A. EWING, F.R.S., M.Inst.C.E. *(20 pp., 5 pls., and 5 text-figs.)*

1908. (March 3.) "On the Physical Aspect of the Atomic Theory." By Professor J. LARMOR, Sec. R.S. *(54 pp.)*

1909. (March 9.) "On the Influence of Moisture on Chemical Change in Gases." By Dr. H BRERETON BAKER, F.R.S. *(8 pp.)*

1910. (March 22.) "Recent Contributions to Theories regarding the Internal Structure of the Earth." By Sir THOMAS H. HOLLAND, K.C.I.E., D.Sc., F.R.S.

SPECIAL LECTURE.

1913. (March 4.) "The Plant and the Soil." By A. D. HALL, M.A., F.R.S.

LIST OF PRESIDENTS OF THE SOCIETY.

Date of Election.	
1781.	Peter Mainwaring, M.D., James Massey.
1782-1786.	James Massey, Thomas Percival, M.D., F.R.S.
1787-1789.	James Massey.
1789-1804.	Thomas Percival, M.D., F.R.S.
1805-1806.	Rev. George Walker, F.R.S.
1807-1809.	Thomas Henry, F.R.S.
1809.	*John Hull, M.D., F.L.S.
1809-1816.	Thomas Henry, F.R.S.
1816-1844.	John Dalton, D.C.L., F.R.S.
1844-1847.	Edward Holme, M.D., F.L.S.
1848-1850.	Eaton Hodgkinson, F.R.S., F.G.S.
1851-1854.	John Moore, F.L.S.
1855-1859.	Sir William Fairbairn, Bart., LL.D., F.R.S.
1860-1861.	James Prescott Joule, D.C.L., F.R.S.
1862-1863.	Edward William Binney, F.R.S., F.G.S.
1864-1865.	Robert Angus Smith, Ph.D., F.R.S.
1866-1867.	Edward Schunck, Ph.D., F.R.S.
1868-1869.	James Prescott Joule, D.C.L., F.R.S.
1870-1871.	Edward William Binney, F.R.S., F.G.S.
1872-1873.	James Prescott Joule, D.C.L., F.R.S.
1874-1875	Edward Schunck, Ph.D., F.R.S.
1876-1877.	Edward William Binney, F.R.S., F.G.S.
1878-1879.	James Prescott Joule, D.C.L., F.R.S.
1880-1881.	Edward William Binney, F.R.S., F.G.S.
1882-1883.	Sir Henry Enfield Roscoe, D.C.L., F.R.S.
1884-1885.	William Crawford Williamson, LL.D., F.R.S.
1886.	Robert Dukinfield Darbishire, B.A., F.G.S.
1887.	Balfour Stewart, LL.D., F.R.S.

* Elected April 28th; resigned office May 5th.

Date of Election	
1888-1889.	Osborne Reynolds, LL.D., F.R.S.
1890-1891.	Edward Schunck, Ph.D , F.R.S.
1892-1893.	Arthur Schuster, Ph.D., F.R.S.
1894-1896.	Henry Wilde, D.C.L., F.R.S.
1896.	Edward Schunck, Ph.D., F.R.S.
1897-1899.	James Cosmo Melvill, M.A., F.L.S.
1899-1901.	Horace Lamb, M.A., F.R.S.
1901-1903.	Charles Bailey, M.Sc., F.L.S.
1903-1905.	W. Boyd Dawkins, M.A., D.Sc., F.R.S.
1905-1907.	Sir William H. Bailey, M.I.Mech.E.
1907-1909.	Harold Baily Dixon, M.A., F.R.S.
1909-1911.	Francis Jones, M.Sc., F.R.S.E.
1911-1913.	F. E. Weiss, D.Sc., F.L.S.
1913-	Francis Nicholson, F.Z.S.

VOL. 57: PART I.

MEMOIRS AND PROCEEDINGS

OF

THE MANCHESTER LITERARY & PHILOSOPHICAL SOCIETY, 1912-1913.

CONTENTS

Memoirs:

MANCHESTER:
36, GEORGE STREET.

Price Three Shillings and Sixpence.

March 25th, 1913.

RECENT ADDITIONS TO THE LIBRARY.

Presented.

Bamberg.—Remeis Sternwarte. i. Katalog und Ephemeriden veränderlicher Sterne für 1912. ii. Katalog...für 1913. By Ernst Hartwig. Leipzig, 1912. (*Recd. 13/xi./12 and 9/i./13.*)

Batavia.—Bataviaasch Genootschap van Kunsten en Wetenschappen. Rapporten van de Commissie in Nederlandsch-Indië voor Oudheidkundig Onderzoek op Java en Madoera. 1911. Batavia, &c., 1912. (*Recd. 21/xii./12.*)

Binney (James). The Centenary of a Nineteenth Century Geologist: Edward William Binney, F.R.S. By James Binney. Taunton, 1912. (*Recd. i./ii./13.*)

Cassel.—Vereins fur Naturkunde. Festschrift des Vereins für Naturkunde zu Cassel zu Feier seines fünfundsiebzigjährigen Bestehens. Cassel, 1911. (*Recd. 20/xii./12.*)

Chicago.—Newberry Library. Narratives of Indian Captivity. Chicago, Ill., 1912. (*Recd. 21/i./13.*)

Desch (Cecil H.). The Crystallization of Metals. By Cecil H. Desch. Glasgow, 1912. (*Recd. 14/xi./12.*)

——. Report on Diffusion in Solids. By Cecil H. Desch. London, 1912. (*Recd. 14/xi./12.*)

London.—Annals of Botany. Vols. I.—XX. London, 1887-1906. (*Recd. 2/x./12.*)

——.—**British Museum.** A Monograph of the Mycetozoa...British Museum. By A. and G. Lister. 2nd ed. London, 1911. (*Recd. 30/x./12.*)

——.— —— A Revision of the Ichneumonidae...By Claude Morley. London, 1912. (*Recd. 30/x./12.*)

——.—**Meteorological Office.** A Barometer Manual... 7th ed. London, 1912. (*Recd. 14/i./13.*)

——.— —— The Effect of the Labrador Current upon the Surface Temperature of the North Atlantic and of the latter upon Air Temperature and Pressure over the British Isles. By C. Hepworth. London, 1912. (*Recd. 16/i./13.*)

——.— —— Further Contributions to the Investigation of the Upper Air. By W. H. Dines. Preface by W. N. Shaw. London, 1912. (*Recd. 16/i./13.*)

——.— —— Graphical Construction for the Epicentre of an Earthquake. By G. W. Walker. London, 1912. (*Recd. 16/i./13.*)

——.— —— On the Radiation Records obtained in 1911 at South Kensington, together with a comparison between them and the corresponding absolute observations of Radiation made at Kew Observatory. By R. Corless. London, 1912. (*Recd. 16/i./13.*)

——.—**Patent Office.** Subject List of Works on Mineral Industries...in the Library of the Patent Office. Part I. (New Series, WN—XN 39). London, 1912. (*Recd. 7/x./12.*)

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MANCHESTER:

36, GEORGE STREET.

Price Four Shillings.

July 29th, 1913.

RECENT ADDITIONS TO THE LIBRARY.

Presented.

Berkeley.—University of California. Library of the University of California. Contents—Index. Vol. I. Berkeley, 1889-1890. (*Recd. 20/iii./13.*)

Delft.—Technische Hoogeschool. Hoekijzerverbindingen in het bijzonder die der Langs aan Dwarsdragers in Bruggen . . . door J. H. A. Haarman. Delft, 1913. (*Recd. 15/vii./13.*)

——.— —— Tektonische und Stratigraphische Beobachtungen am südwestrande des Limburgischen Kohlenreviers. Von W. C. Klein. Amsterdam, n.d. (*Recd. 15/vii./13.*)

——.— —— Snelheidsmetingen bij de Reactie van Friedel en Crafts . . . door S. C. J. Olivier. Delft, 1913. (*Recd. 15/vii./13.*)

——.— —— Overheidsbemoeiingen met Stedebouw tot aan den Vrede van Munster . . . door W. B. Peteri. Alkmaar, 1913. (*Recd. 15/vii./13.*)

——.— —— Bijdrage tot de Kennis der Katalyse . . . door H. J. Prins. Amsterdam, n.d. (*Recd. 15/vii./13.*)

——.— —— Het Sociale Arbeidscontract . . . door J. van Hettinga Tromp. Amsterdam, 1913. (*Recd. 15/vii./13.*)

——.— —— Over eenige factoren, die de ontwikkeling van Penicillium glaucum beïnvloeden . . . door H. I. Waterman. n.p., n.d. (*Recd. 15/vii./13.*)

Lebon (Ernest).—Notice sur Henri Poincaré. Paris, 1913. (*Recd. 15/ii./13.*)

Leicester.—Literary and Philosophical Society. The Keuper Marls around Charnwood. By T. O. Bosworth. Leicester, n.d. (*Recd. 26/iii./13.*)

London.—Patent Office. Guide to the Search Department of the Patent Office Library, with Appendices. 4th ed. (Patent Office Library Series, No. 4). London, 1913. (*Recd. 17/ii./13.*)

Madison.—State Historical Society of Wisconsin. Annotated Catalogue of Newspaper Files in the Library of the State Historical Society of Wisconsin. 2nd ed. By A. T. Griswold. Madison, 1911. (*Recd. 20/iii./13.*)

——.— **Wisconsin History Commission.** Civil War Messages and Proclamations of Wisconsin War Governors. Ed. by R. G. Thwaites. (Wisconsin History Commission: Reprints, No. 2.) n.p., 1912. (*Recd. 4/iii./13.*)

——.— —— A Narrative of Service with the Third Wisconsin Infantry. By J. W. Hinkley. (Wisconsin History Commission: Original Papers, No. 7). n.p., 1912. (*Recd. 4/iii./13.*)

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MEMOIRS AND PROCEEDINGS

OF

THE MANCHESTER LITERARY & PHILOSOPHICAL SOCIETY, 1912-1913.

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MANCHESTER:
36, GEORGE STREET.

Price Ten Shillings and Sixpence.

December 31st, 1913.

RECENT ADDITIONS TO THE LIBRARY.

Presented.

Batavia.—**Bataviaasch Genootschap van Kunsten en Wetenschappen.** Rapporten van de Commissie in Nederlandsch—Indië voor Oudheidkundig Onderzoek op Java en Madoera. 1912. Batavia, etc., 1913. (*Recd.. 20/ix./13.*)

Chicago.—**Newberry Library.** Descriptive account of the collection of Chinese, Tibetan, Mongol; and Japanese Books in the Newberry Library. By B. Laufer. (Publications of the Newberry Library, No. 4.) Chicago, Ill., 1913. (*Recd. 21/vi./13.*)

Formosa.—**Government of Formosa. Bureau of Productive Industries.** Acrididen Japans. By T. Shiraki. Tokyo, 1910. (*Recd. 29/x./13.*)

——.— —— Monographie der Grylliden von Formosa. By T. Shiraki. Taihoku, 1911. (*Recd. 29/x./13.*)

London.—**British Museum (Natural History).** The History of the Collections contained in the Natural History Departments of the British Museum. Vol II.-Appendix. By Albert Günther. London, 1912. (*Recd. 9/viii./13.*)

——.— —— A Revision of the Ichneumonidæ based on the Collection in the British Museum (Natural History). Part II. By Claude Morley. London, 1913. (*Recd. 9/viii./13.*)

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Cardiff.—Naturalists' Society. Transactions.

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Calcutta.—Indian Association for the Cultivation of Science. Bulletins.

The List of Exchanges and Periodicals has been amended as follows:—

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Tokyo.—Imperial University Journal of the College of Agriculture.

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——.— **Registrar-General's Annual Report...,** The.

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